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Vice, Virtue, and the Vedalia

By Richard L. Doutt

Department of Biological Control

University of California, Albany

Although the biological control method has a history antedating the introduction of *Rodolia* (*Vedalia*) *cardinalis* (Mulsant) into California in 1888, it is everywhere agreed that this project against the cottony-cushion scale, *Icerya purchasi* (Maskell), established the procedure as a valid method of pest control. It was not unlike some of the other brilliant, spectacular achievements in science that have marked a sudden and general recognition of a new concept. The time and the setting formed ideal conditions in which the catalytic genius of the men involved could set off this magnificent demonstration of applied ecology. The event has been noted many times, but it bears repeating not merely because of its importance to economic entomology but primarily because it is one of the most fascinating tales in entomological history. It contains much of human interest, and includes such surprisingly non-entomological items as an unhappy love affair, political intrigue, and a pair of diamond earrings.

There is much more to the story than appears in most references to the subject. The present version is the result of objective research through old records, correspondence, and newspaper clippings seventy years after the notable event, the birth of biological control.

In 1887 the infant citrus industry in California was threatened with destruction because a massive infestation of cottony-cushion scale was forcing the farmers to abandon citrus growing as a commercial venture. The Convention of Fruit Growers meeting in Riverside, California, in April 1887, invited as their principal speaker Charles Valentine Riley, Chief of the Division of Entomology of the federal government, to whom the citrus growers looked to provide a remedy.

The Career of Riley:—Riley was a colorful man. Born in London, he had been forced to emigrate to America at the age of 17 because of financial reverses. After a period of hardship and after working on a farm, as a reporter, and serving as a private in the Civil War, his ability was recognized, and at the age of 25 he was appointed State Entomologist of Missouri.

In 1874–1876 when some of the western states suffered greatly from an outbreak of the Rocky Mountain locust, Riley, through his forceful personality, persuaded Congress to appropriate money to establish the U. S. Entomological Commission, of which he was made Chief, to investigate the problem.

According to one biographer (Howard, 1930) who knew him well, Riley was a restless man, ambitious to build up a large organization. He was a great schemer, striving constantly to make his work appear important. His aggressive personality was bound to make enemies. As an administrator he was sometimes tactless and would write to one subordinate about the supposed shortcomings of some other member of his staff. His disposition was undoubtedly affected by his health; he had frequent headaches and was troubled with insomnia. He could not sleep well in his own bed at home, but slept soundly on long railway journeys or in a barber's chair; after a sleepless night he would often go to his barber and pay by the hour to make up lost sleep.

But as an entomologist Riley had become such a national figure by 1878 that he was appointed Entomologist for the U. S. Department of Agriculture and Chief of the Division of Entomology.

From his work with the U. S. Entomological Commission he was accustomed to collaring congressmen for appropriations, and followed the same practice in his new post. He felt entomology needed more money, which it did, and that C. V. Riley deserved more salary, which he did. He would go directly to congressmen for these funds and didn't bother to channel his requests through his superiors in the Department of Agriculture. Such unorthodox procedure resulted in his resignation in 1879 before he had served a year for the U.S.D.A. (His replacement was the famous Professor Comstock of Cornell). However, a year later Garfield was elected President and the political pressure brought about by the change of administration carried Riley back into office shortly after Garfield's inauguration in March of 1881.

Koebele and Coquillett:—When Riley addressed the Fruit Growers' Convention at Riverside in 1887 he had two field agents working in California. One was Albert Koebele, a naturalized German immigrant whom he had first met at a meeting of the Brooklyn Entomological Society. Riley had been impressed by the beautiful mounting of specimens that Koebele exhibited in a collection and invited Koebele to come to Washington, D. C., and work for Riley at the Division of Entomology. Koebele seems to have been so overwhelmed by this invitation that he immediately gave up his job and got ready to go to Washington. There appears to have been several agonizing months of waiting, and finally in October 1881, he received a letter from Riley saying, "I have felt aggrieved at having been the unintentional instrument in causing you to give up your former position before I had secured one for you in Washington." Riley then offered him a position as a temporary agent at $65 per month, and added, "I hope you will bring along all the specimens possible."

In 1885, after what is said to have been "an unfortunate love affair" (Howard, 1930), Koebele asked to be transferred to some distant place. At that time the most distant place was California so Riley sent him there on a salary of $100 per month on a mission officially designated as "Investigation of the history and habits of insects of California." Riley wrote out explicit instructions to Koebele including this point: "It is distinctly to be understood that you are not to collect for any other person or institution, or to make a private collection for yourself, but all material is to be sent on to the Department." Koebele liked Alameda, and Riley permitted him to operate from this base most of the time. The correspondence during this period indicates that Koebele had difficulty with the expense account rules, for there were many arguments over items submitted and threats to disallow them.

The other agent in California was D. W. Coquillett, a native of Illinois. He was an entomologist by choice, had taught school in the middle west, but in 1882 after contracting tuberculosis had moved to Anaheim, California, to regain his health. His publications in entomology attracted the attention of Riley who appointed him as field agent in 1885.

Coquillett was stationed most of the time around Los Angeles and was assigned to work on control of the cottony-cushion scale. Koebele was sent down to work with him in February of 1886. Koebele's instructions were "to assist Mr. Coquillett in every way that you can, not as his assistant, but as his associate. You both together will be able to do good work and you will have plenty of opportunity to collect as much as you wish."

Koebele soon became critical of Coquillett and wrote his views to Riley. This may have influenced Riley to lay off Coquillett for about a year, although insufficient funds

was the reason officially offered. Koebele, however, was raised to $1,500 per year.

Coquillett was rehired in 1887 but in the meantime had conducted some private experiments on HCN fumigation for scale control along with Alexander Craw, who was foreman of the ranch of J. W. Wolfskill at Los Angeles.

Riley's Plan to Control Cottony-Cushion Scale:—Riley's address to the Fruit Growers' Convention contained the following major points:

1. The original home of *Icerya purchasi* was stated as Australasia, but he was not sure whether it was Australia or New Zealand.

2. He believed that the scale had been accidentally introduced to California at Menlo Park on Acacia in 1868, but he was not certain of this as he had learned that all Acacias had been brought in as seed.

3. No parasites had hitherto been reported, but from specimens taken at Los Angeles he had reared two Chalcids which L. O. Howard had named *Isodromus iceryae*. (This comment is particularly interesting for two reasons. We know now that *I. iceryae* is an encyrtid parasite of *Chrysopa*, not cottony-cushion scale. Also an Australian, Frazer Crawford, had written Riley earlier in the year telling him that *Icerya* in Adelaide had been destroyed by a dipterous parasite. He enclosed drawings of the fly. Crawford wrote that "this is the only instance I know of a true Dipteron being a coccid parasite." Riley suggested that Crawford meant a predator when he said parasite, and Riley also remarked that this type of fly had its larval stage in the ground. Crawford's fly was the famous *Cryptochaetum iceryae* (Williston), a valuable parasite of *Icerya!*)

4. Riley somewhat grudgingly admitted that HCN fumigation might have possibilities. However, it rankled Riley that this had not been developed under his supervision. He said, "We cannot perhaps blame the gentlemen for endeavoring to realize something out of what they consider to be a valuable discovery that will compensate them for the time they have devoted to the purpose; but I am always supicious of secret or patent insect remedies. My friend, Mr. Coquillett, perfected this gas after his employment by the Department of Agriculture ceased. But it is a general truth that the moment any person or persons become interested in a patent or in any remedy they desire to control, from that moment their judgment can no longer be depended on as to value of other remedies."

5. He indicated that Coquillett and Koebele had disagreed on the effectiveness of certain washes (sprays) they had experimented with. Coquillett believed in kerosene emulsions while Koebele believed in the efficacy of resin soaps.

6. Finally, Riley said, "It had doubtless occurred to many of you that it would be very desirable to introduce from Australia such parasites as serve to keep this fluted scale in check in its native land. . . . This State—yes, even Los Angeles County—could well afford to appropriate a couple of thousand dollars for no other purpose than the sending of an expert to Australia to devote some months to the study of these parasites there and to their artificial introduction here. But the agent must be an expert entomologist and his selection should be left to some competent authority."

"I would not hesitate, as United States Entomologist, to send some one there with the consent of the Commissioner of Agriculture were the means for the purpose at my command; but unfortunately, the mere suggestion that I wanted $1,500 or $2,000 for such a purpose would be more apt to cause laughter and ridicule on the part of the average committee in Congress than serious and earnest consideration, and the action of the last Congress has rendered such work impossible by limiting investigation to the United States."

(This last comment referred to a rider put on the appropriation bill for the Department of Agriculture for that year. It prohibited foreign trips by employees and was purposely inserted in the bill to stop the frequent European junkets taken by Riley.)

As a result of Riley's address and persuasion the Convention adopted a resolution favoring the idea of sending someone to Australia to seek out natural foes of *Icerya* and to bring them to California if any were found. (Pacific Rural Press, Jan. 7, 1888).

Search for the Scale's Native Home:—During this time, Waldemar G. Klee, a native of Denmark, had in 1886 left his job as head gardener of the experimental grounds of the University of California's College of Agriculture at Berkeley to accept a position as State Inspector of Fruit Pests. Klee had been corresponding with W. M. Maskell in New Zealand and Frazer Crawford in Australia about cottony-cushion scale. In the May 7, 1887 issue of the Pacific Rural Press a letter from Maskell to Klee was published in which Maskell stated positively that the *Icerya* was native to Australia. He pointed out that it had appeared at the Cape of Good Hope several years earlier and that the people there called it the "Australian Bug". It was thought to have been carried there by people going to the diamond fields from Sydney or Melbourne.

On June 4, 1887 the Pacific Rural Press published an article by Riley noting the published correspondence between Maskell and Klee. In this article Riley pointed out that it was important to locate the native home of a pest, and proceeded to discard the idea that *Icerya* came from Australia. All the evidence, he said, pointed to the fact that *Icerya* was from the Island of Mauritius in the Indian Ocean.

Riley said that sugar was imported from Mauritius all over the world, and that it was customary to cover the top of every hogshead of sugar with pieces of cane. In this sugar there is an insect known to the trade as the sugar louse and Riley was sure that this was *Icerya*. Koebele was then directed by Riley to go to sugar refineries around San Francisco and collect some of the sugar lice to prove Riley's point. Koebele did make such a search and explained to Riley that the sugar people had pointed out that the sugar louse was actually a mite, an acarid. This made Riley a little annoyed at Koebele and he repeated that what he was looking for was *Icerya*.

However, by March 4, 1888 Riley had altered his position once more and wrote in the Pacific Rural Press that the view that *Icerya* was native to Australia was probably the correct one.

Koebele's Mission to Australia:—Meanwhile the California growers had been putting pressure on their Washington representatives to get action. In a letter to Representative Felton (Pacific Rural Press, Feb. 11, 1888) Norman J. Colman, Commissioner of Agriculture, pointed out that while travel by the Division of Entomology was restricted to travel in the United States, there was to be an International Exposition in Melbourne in which the United States Government was to take part. In a very pregnant statement Colman said, "This exposition in many ways would further the investigation referred to in your memorial." The opportunity to exploit the appropriation for the Melbourne Exposition was not overlooked. The Hon. Frank McCoppin of San Francisco was made U. S. Commissioner to the Exposition and he arranged with other members of his Commission to set aside $2,000 to pay the expenses of an entomologist who was to accompany them, ostensibly to represent the State Department but actually to collect enemies of *Icerya*.

Riley selected Koebele to accompany the Commission, although the California people had suggested Coquillett. According to his letters, Riley himself had intended to follow Koebele but his trip never materialized. Koebele sailed for Australia on August 25, 1888.

Koebele had scarcely set foot on Australia when Riley started being suspicious of the California people and began

to guard jealously any loss of credit. He wrote the following to Koebele:

"I have been somewhat surprised at receiving an intimation from Hilgard and others in California to the effect that they understand that you are poorly supplied with funds and that the government is not going to be able to back you properly from a pecuniary standpoint. There seems, as you must be aware, a tendency on the part of the California people to push things forward in this matter as much as possible and not only to assist but probably also with the idea of claiming credit for the larger share of it. Now I wish you to remember that this is exclusively a government matter. In the first place did they get this impression from anything you said or did before leaving California? In the second place by no means must you help to further this impression. You are a government agent and this office has full charge of this expedition and I do not wish to have anyone else connected in any way with your mission or its results. You must therefore be very careful in what you do and particularly in what you write to California. All material which you send on in advance of your return must be sent to Coquillett for him to place and to on one else." (Apparently Koebele didn't know how much was set aside for his trip as Riley penciled a foot note indicating $2,000 were available.)

Jealousy of credit was not, however, limited to Riley. In 1887, Frazer Crawford who had discovered the *Cryptochaetum* in 1886, sent some of these flies to Klee who liberated the material in San Mateo County in 1888, before Koebele sailed for Australia. (Today *Cryptochaetum* is a very effective enemy of *Icerya* in San Mateo and neighboring counties around San Francisco Bay.) A Los Angeles newspaperman became aware of the fact that three men (Coquillett, J. W. Wolfskill, owner of the Wolfskill ranch; and his foreman Alexander Craw) were soon to be handling parasites of *Icerya* and wrote an article praising them and remarking that they would probably do more for Southern California than had been accomplished in many years. This account came to the attention of Crawford in Australia and wounded his pride for he felt he was being denied the credit and recognition to which he was entitled. He wrote: "All honor, then, be to this patriotic trio, and personally let me express my compliments to the writer of the article, because until I read it I labored under the delusion that I first discovered the Dipteron, that I first suggested its introduction into California and other countries afflicted by the *Icerya* scourge, and I have put myself to some little and my friends to much greater trouble in collecting and forwarding the coccid hosts of these parasite flies—all of which doubtless is a mistake."

The Introduction of the Vedalia to California:—In Australia, Koebele went to work. In October 1888, he wrote Riley as follows, "So far my work has been much more successful than I expected. I not only found the dipterous parasite within *Icerya* in large numbers, but also three predaceous larvae feeding upon the eggs of *Icerya*. One of these is a Chrysopa larva;—*the others are larvae of a small coccinella.*" (Italics mine.)

How perilous it is to make predictions about imported species before they are tested, is illustrated in Riley's reply to Koebele's letter that "the sending of the coccinellids is of course desirable but I think we have much more to hope from the (*Cryptochaetum*)".

Through Koebele's efforts a total of approximately 12,000 individuals of *Cryptochaetum* were sent to California, and relatively little attention was at first paid to his discovery of the vedalia feeding upon the *Icerya* in a North Adelaide garden on October 15, 1888. The actual introduction of the vedalia is best described in these words of Coquillett:

"The first consignment of these lady-birds reached me on the 30th of November (1888), and numbered 28 specimens; the second consignment of 44 specimens arrived December 29; and the third consignment of 57 specimens reached me on January 24, making 129 specimens in all. These, as received, were placed under a tent on an *Icerya*-infested orange tree, kindly placed at my disposal by J. W. Wolfskill, of this city (Los Angeles). Here they were allowed to breed unmolested, and early in April it was found that nearly all of the *Iceryas* on the enclosed tree had been destroyed by these voracious lady-birds. Accordingly, on the 12th of April, one side of the tent was removed, and the lady-birds were permitted to spread to the adjoining trees. At this date I began sending out colonies to various parts of the state, and in this work have been greatly aided by Wolfskill and his foreman, Alexander Craw, both of whom were well acquainted with the condition of the orchards in this part of the state. By the 12th of June we had thus sent out 10,555 of these lady-birds, distributing them to 228 different orchardists; and in nearly every instance the colonizing of these lady-birds on *Icerya*-infested trees in the open air proved successful. The orange and other trees—about 75 in number—and also the shrubs and plants growing in Wolfskill's yard, have been practically cleared of *Iceryas* by these lady-birds, and the latter have of their own accord spread to the adjoining trees to a distance of fully three-fourths of a mile from the original tree."

Coquillett then continues to describe the distribution of the vedalia in California:

"Besides the three consignments of these lady-birds referred to above I also received two later consignments. The first of these reached me February 21, and numbered 35 specimens; these I colonized on an *Icerya*-infested orange tree in the large orange grove belonging to J. R. Dobbins, of San Gabriel. The last consignment of 350 specimens arrived March 20; one-third of these I left with Dobbins, while the remainder I colonized on orange trees in the extensive grove owned by Messrs. A. B. and A. Scott Chapman, in the San Gabriel Valley. All of these colonies have thrived exceedingly well. During a recent visit to each of these groves I found the lady-birds on trees fully one-eighth of a mile from those on which the original colonies were placed, having thus distributed themselves of their own accord. The trees I colonized them on in the grove of Dobbins were quite large and were thickly infested with the *Iceryas*, but at the time of my recent visit scarcely a living *Icerya* could be found on these and on several adjacent trees, while the dead and dry bodies of the *Iceryas* still clinging to the trees by their beaks, indicated how thickly the trees had been infested with these pests, and how thoroughly the industrious lady-birds had done their work."

On July 2, 1889 citrus grower Dobbins had much to say about the work of the vedalia on his property:

"The Vedalia has multiplied in numbers and spread so rapidly that every one of my 3,200 orchard trees is literally swarming with them. All of my ornamental trees, shrubs, and vines which were infested with white scale, are practically cleansed by this wonderful parasite. About one month since I made a public statement that my orchard would be free from *Icerya* by Nov. 1, but the work has gone on with such amazing speed and thoroughness that I am today confident that the pest will have been exterminated from my trees by the middle of August. People are coming here daily, and by placing infested branches upon the ground beneath my trees for two hours, can secure colonies of thousands of the vedalia, which are there in countless numbers seeking food. Over 50,000 have been taken away to other orchards during the past week, and there are millions still remaining, and I have distributed a total of 63,000 since June 1. I have a list of 130 names of persons who have taken colonies, and as they have been placed in orchards extending from South Pasadena to Azusa, over a belt of country 10 miles long and six or seven in width, I feel positive from my own experience, that the entire valley will be practically free from *Icerya* before the advent of the New Year."

On July 31, 1889 Dobbins' orchard was so completely clean of *Icerya* that he posted a notice saying that he had no more vedalias for distribution. The Los Angeles County Board of Horticultural Commissioners propagated the vedalia for distribution by caging five large orange trees heavily infested with *Icerya* and permitting the vedalia to increase within the enclosures. It is said that scores of people came each day, singly and in groups, with pill-boxes, spool-cotton boxes, or some sort of receptacle in which to place the beetles so that they could be carried home and placed in trees and on vines infested with the scale.

Mr. A. Scott Chapman, who a year earlier had stated that the *Icerya* was forcing him to abandon fruit growing, reported on October 18, 1889 that the vedalia had cleared the scale from 150 acres of his land. The shipments of oranges from Los Angeles County jumped in one year from 700 to 2000 cars. The vedalia was hailed as a miracle of entomology. The cost of the project was about $1500.

During this period a very interesting case of litigation arose in Los Angeles which completely failed to acknowledge the good work of the vedalia. Coquillett reported this in the Pacific Rural Press (Feb. 22, 1890, p. 187): "It appears that a certain party offered to rid a number of orange trees of the *Iceryas* by simply introducing a chemical compound of some sort into the trunk of each tree; but if not successful, was to receive no compensation whatever for his trouble. Accordingly, the trees were duly treated, and a few days later one of the local inspectors of fruit pests, not knowing the facts given above, placed several of the vedalias on these trees. After the lapse of a few weeks, the operator in company with the owner went out and examined the *Iceryas* on the inoculated trees, and found the scale dead and dry; the operator, of course, claimed that his inoculating the trees was what had caused the death of the scales, and therefore demanded his money, but the owner of the trees believed that the vedalias had accomplished this work, evidences of their former presence being still present in the shape of cast-off pupal skins that adhered to various parts of the trees. The case was finally taken to court, and after hearing the evidence the jury decided that the scales had been destroyed by the agency of the chemical substance with which the trees had previously been inoculated." Although this case did not do much for biological control it at least shows that the idea of systemics is not particularly new.

The Fight about the Credit:—By the time Koebele returned from Australia, the vedalia had become a tremendous success. Koebele was given very favorable publicity, and there was talk of creating a position of State Entomologist for him. He went to Washington, D. C. to report to Riley, and there he got a big laugh from his audience when he told them that McCoppin, the U. S. Commissioner to the Melbourne Exposition, wanted the vedalia to be named after him, to be called *McCoppiensis*. This statement of Koebele figured prominently in the long and bitter controversy that began to be waged between Riley and the people in California.

McCoppin, as Commissioner, was charged with the responsibility of writing a report on the Melbourne Exposition for Congress. He wanted to include Koebele's report, but Riley insisted that it be submitted through him. McCoppin accused Riley of trying to "Rileyize" the report. Koebele found himself caught in the middle, and made a pitiful effort to straddle the fence. Riley, to embarass McCoppin, published the statement that McCoppin wanted the vedalia named after him. To this McCoppin replied, "After the Prince of Wales testified in a celebrated divorce case, all England said he lied like a gentleman. I can not say as much for the professor. Like most other men I presume I have my vanities, but they do not lie in that direction. No one in this community (San Francisco) would believe for a moment that a man who had a chief part in laying off and naming all the principle streets of this city, between Divisadero and the Pacific Ocean, and yet did not give his name to one of them, would now sigh to have it associated with a bug, however useful the latter might be." (The present map of San Francisco shows that the name McCoppin has now been commemorated in one street near the U. S. Mint and in one small square.)

Riley asked Koebele to come to his defense, and tell how he had reported that McCoppin wanted the vedalia named after him, but Koebele had a remarkable lapse of memory and said he couldn't remember the incident at all. Commenting on this feeble excuse, Riley wrote: "This episode of the controversy annoys me for it is the only point on which McCoppin has the advantage, and that I should have been placed in so awkward a position by my most trusted agent is lamentable. . . . It will ever be to me, Koebele, a source of astonishment that you should forget a thing of this sort, when the circumstances are so clearly recollected here, and even Miss Kramer recollects the hearty laugh that we had over your statement."

Riley was obsessed with the idea that he was being deprived of credit and it shows clearly in all his letters and articles of the period. For instance, this paragraph from a letter to Koebele:

> "I want your part to be fully appreciated, Koebele, but I also expect you when occasion offers to remember the engineer at headquarters without whom nothing would have been done. . . . So far as I have been able to see not a word of praise or credit has been published . . . . while McCoppin, Coquillett (who deserved but little) and yourself (who deserves the most) are everywhere mentioned and honored."

Riley was building a new house and at first was going to call it Vedalia. Then he changed his mind. "I have concluded to call my new house not Vedalia, but Sunbury. It would look a little too egotistical to call it Vedalia."

During this period when Riley and McCoppin and others were airing their dispute in the pages of the Pacific Rural Press, Koebele kept silent, and it was the much maligned Coquillett who jumped in to support his chief. Nevertheless, Koebele was continually writing Riley about Coquillett's faults, and Riley was agreeing. He wrote to Koebele, "I am entirely of your mind in reference to the unsatisfactory experiment of Coquillett on these important enemies of *Icerya*. I would give him a good rap over the knuckles publicly did I not fear it would hurt our work in California if I did so."

Koebele was skyrocketed to fame in California by his Australian trip. The Germans for a time called biological control the Koebele method. The California fruit growers, stimulated by Lelong, then Secretary of the State Board of Horticulture, drafted resolutions thanking Koebele and McCoppin for their work. In appreciation a fund was started, and enough was raised to present Koebele a gold watch, and Mrs. Koebele a pair of diamond earrings.

Dr. L. O. Howard wrote (1925): "I imagine that this presentation of diamond earrings to the wife of an entomologist as the result of his own work in entomology is probably unique. I happen to know of no other entomologist who has been able to see that his wife wears diamond earrings."

In 1891 the California legislature appropriated $5,000 to send an entomologist on another expedition to Australia for other parasites of scale pests. Lelong wrote to the Secretary of Agriculture to make this a cooperative enterprise. He proposed that the U. S. D. A. pay the man's salary, thus the $5,000 would go much farther. Koebele's salary was then $1,500 per year. At first Riley turned down this request. However, shortly afterward, Rusk, the Secretary of Agriculture, was persuaded by California people, to accept the proposition, Riley was forced to yield, and Koebele went on another expedition. Riley's dislike of McCoppin now turned toward Lelong and he became increasingly bitter toward the California situation. Koebele fell more and more in his disfavor, and Coquillett began to increase in his estimation. When Koebele wrote another

criticism of Coquillett in 1892, Riley replied that "your prejudices are without foundation. I hope your next letter will be a more sober one."

The friction between Riley and the California people increased to such an extent that in September 1893 Riley recalled his agents from California. Coquillett returned to Washington, but Koebele had apparently seen the change coming and had corresponded with the Hawaiian government about employment. When Riley recalled him, he immediately resigned and went to work for Hawaii at a tremendous salary increase ($250 per month and expenses).

It may be of interest to note what became of these men after they left the California scene. Koebele worked in Hawaii until 1912, then returned to his native home in Germany, where he was caught by the first World War. After the war he lived in failing health in the terrible conditions of post-war Germany. He died in 1924, never having been successful in his attempt to regain entry into the United States. It has been said of Koebele that he was one of the best collectors, and one of the most skilled preparators that ever lived, and the results of his work were of great value as we have seen.

Riley retired from his position as Chief in 1894 and in the next year died as a result of injuries received when he fell from his bicycle. Certainly Riley was a remarkable man and an outstanding entomologist. He published 2,418 papers under his name and 478 additional ones with his mentor, B. D. Walsh. Howard (1930) thought Riley's five greatest contributions were:

1. His Missouri Reports which were unlike anything in entomology before.
2. His efforts in founding the U. S. Entomological Commission which was the first broad, national investigation of insect plagues.
3. His establishment of a strong federal organization of entomology.
4. His work with *Phylloxera*.
5. His role in the vedalia project.

Coquillett's contributions to entomology are also very sound. He not only was an early pioneer in practical work with fumigation, sprays, and biological control but he was a good taxonomist. He published 258 papers, and from 1896 until his death in 1911 was honorary custodian of Diptera at the U. S. National Museum, and was at the time perhaps America's leading Dipterist.

Since the suppression of *Icerya* in California projects in biological control have been conducted all over the world, and many of these have been equally successful and effective. None, however, has equalled its drama nor its appeal to the public's fancy. It has remained *the* project in biological control and an important milestone in applied entomology.

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NEMATOLOGY AT THE UNIVERSITY OF FLORIDA

Dr. Vernon G. Perry, formerly of the Agricultural Research Service, U. S. Department of Agriculture at the University of Wisconsin, has been appointed professor of Nematology and Nematologist in the Department of Entomology, College of Agriculture, University of Florida. Dr. Perry is to report for duty on January 1, 1959. He will be responsible for graduate and undergraduate courses in nematology and will conduct research for the Florida Agriculture Experiment Station.

HONORARY MEMBERS

ENTOMOLOGICAL SOCIETY OF AMERICA

In the 1958 election, E. O. Essig and W. C. O'Kane were added to the Honorary Membership of the Society, making a total of twelve in this category. Interested members have suggested that an annual publication of the list of Honorary Members would be of interest.

The Constitution provides that the Society may have no more than 15 Honorary Members and that no more than two can be added in a given year. This is therefore a most select group of highly honored scientists which we are proud to commend to your attention.

PHILIP P. CALVERT
P. O. Box 14
Cheyney, Pennsylvania

ERNEST N. CORY
Department of Entomology
University of Maryland
College Park, Maryland

A. DA COSTA LIMA
Instituto Oswaldo Cruz
Cx. Postal 926
Rio de Janeiro, Brazil

J. J. DAVIS
Department of Entomology
Purdue University
Lafayette, Indiana

E. O. ESSIG
Department of Entomology
University of California
Berkeley, California

STUART W. FROST
465 E. Foster Avenue
State College, Pennsylvania

O. A. JOHANNSEN
Box 48, College of Agriculture
Ithaca, New York

W. C. O'KANE
Durham, New Hampshire

WILLIAM A. RILEY
Department of Zoology
University of Minnesota
Minneapolis, Minnesota

V. E. SHELFORD
Vivarium Building
Wright & Healey Streets
Champaign, Illinois

R. E. SNODGRASS
Division of Insects
U. S. National Museum
Washington 25, D. C.

EDMUND M. WALKER
Royal Ontario Museum of Zoology and Paleontology
100 Queen's Park
Toronto 5, Ontario, Canada

ENTOMOLOGICAL SOCIETY OF SOUTHERN AFRICA

The 21st Annual General Meeting of the above Society was held at Pretoria, South Africa on August 8, 1958. The Society now has a total of 291 members and permanent subscribers. Dr. E. McC. Callan's Presidential Address entitled "Some Aspects of the Geographical Distribution of Insect Pests" was read at the meeting. Dr. Callan, who is the retiring President, has been a member of the Entomological Society of America for the past 15 years. He is also a Fellow of the Royal Entomological Society of London, a member of the Society for British Entomology, and a member of the Hawaiian Entomological Society. Newly elected President of the Entomological Society of Southern Africa is Dr. B. Smit of Pretoria.

B & Z Library

Zoogeography and Evolution of Pacific Insects

(A review of the entomological program at the Bernice P. Bishop Museum)

By J. L. GRESSITT AND L. W. QUATE[1]

After five years of expansion in the entomological activities at the Bernice P. Bishop Museum, it seems appropriate to review the progress made to date and evaluate the program and its objectives.

The ultimate goal of this program is the understanding of the zoogeography and evolution of Pacific insects, i.e., how and when the insects spread from the "source areas" to other areas, and evolutionary changes which have occurred during and after population movements (correlated, of course, with knowledge available from vertebrate zoogeography and the physical factors shown by geology, climatology and oceanography). To attain this objective the following steps are necessary:

1. the amassing of documented collections of insects and their close relatives from all islands of the Pacific Ocean, and, as appropriate, from the zoogeographically related areas of Asia and Australia;
2. ecological investigations in the field program where possible;
3. the conduct of research on the accumulated materials, both by the Museum staff and collaborators at other institutions;
4. the publication of results, aiming primarily at systematic monographs of specific groups of insects from the South Pacific area or portions thereof and conclusions regarding zoogeography and evolution of Pacific insects.

HISTORICAL SKETCH

Bishop Museum's first major acquisition of insects was from the Perkins' collection, some forty years ago. R. C. L. Perkins was sent to the Hawaiian Islands by the Royal Society of London and the British Association for the Advancement of Science from 1892 to 1913 to collect and study the insect fauna of the Hawaiian Islands. In return for financial assistance with the resultant three volume monograph, "Fauna Hawaiiensis," by Perkins and other leading entomologists, the Museum acquired one-third of the material collected, amounting initially to 12,500 specimens and with later additions from Perkins' own collections.

The appointment of O. H. Swezey as Honorary Curator of Entomology in 1907 marked the commencement of an entomology department at the Bishop Museum. Dr. Swezey, staff member of the Hawaiian Sugar Planters' Association Experiment Station, gave freely of his services for nearly fifty years and contributed an immense amount of time and effort in adding to the Museum's insect collections and building up its entomology department.

The Perkins' expedition has been the only one organized specifically to collect Hawaiian insects over an extended period of time, but many other entomologists have contributed to the Museum's Hawaiian collections. O. H. Swezey, J. W. Beardsley and others of the H.S.P.A. Entomology Department; D. T. Fullaway, N. L. H. Krauss, P. W. Weber, Territory of Hawaii Board of Agriculture and Forestry; J. F. Illingworth, first with the Museum and later with the Pineapple Research Institute; E. H. Bryan, Jr., Bishop Museum; E. C. Zimmerman and R. L. Usinger, formerly of the Museum; D. E. Hardy, University of Hawaii; W. M. Giffard, local businessman and amateur taxonomist; and E. J. Ford, Jr., U. S. Dept. Agr., Plant Quarantine, have been largely responsible for the continued growth of local collections. The present entomological staff attempts to add to this whenever time permits. The Museum now has a good representative collection of Hawaiian insects, although it is realized that much remains to be done in the Islands before the insect fauna will be completely known. This is especially true for islands other than Oahu.

As the Museum grew, its interests expanded to include the whole Pacific area. Zoological expeditions were sent to the islands of the Pacific and a rich collection of insects has resulted. Being a small institution, the Bishop Museum has had to rely on much outside financial assistance and has been fortunate in obtaining splendid cooperation from a number of local and mainland organizations which support scientific research.

Early ventures included the Tanager Expedition to the northwestern Hawaiian islands, Johnston and Wake Island (1923), the Whippoorwill Expedition to the Equatorial Islands (1924), and the Whitney South Seas Expedition (1924). The latter was the start of international cooperation by specialists on Pacific insects. The fruitful results gained by this method prompted future studies to follow a similar pattern. The insects collected on Samoa by E. H. Bryan were pooled with those collected by the British entomologists P. A. Buxton and G. H. E. Hopkins on Upolu and studied by a group of specialists from American, British, and European institutions. The research was published by the British Museum as the "Insects of Samoa."

The Pacific Entomological Survey was undertaken as a joint venture of the Museum, Hawaiian Sugar Planters' Association and the Association of Hawaiian Pineapple Canneries (now known as the Pineapple Research Institute) to collect the insects of the Pacific Islands. In 1928, two entomologists, E. P. Mumford and A. M. Adamson, landed in Tahiti and with native assistants spent about one and one-half years in the Marquesas and Society Islands. Various complications and high costs caused the Survey to fall short of the ambitious goals set for it and it was terminated in 1933. As background for the Survey, compilation of a bibliographic file of Pacific insects was undertaken by Amy Suehiro, who has since continued entomological work at the Museum.

The most extensive pre-war project was the Mangarevan Expedition in 1934 under the leadership of C. Montague Cooke, Jr. The personnel included men trained in entomology, malacology, botany, and anthropology. During six and one-half months the party traveled 9,000 miles and visited four of the Line Islands, all of the principal Society Islands, nine atolls in the Tuamotu Archipelago, all the Mangarevan Islands, Pitcairn, Rapa, and all the islands of the Australs. E. C. Zimmerman, the entomologist, collected more than 40,000 specimens for the Museum. 40 Occasional Papers and several Bulletins have been published on the material.

Dr. Zimmerman became Entomologist at the Museum on his return and filled that position until 1935, and also 1936–46. During that time he began the monumental work, "Insects of Hawaii," which is being continued at the present time and is in the eighth volume. It is being published by the University of Hawaii Press.

R. L. Usinger replaced Zimmerman in 1935 for one and one-half years before returning to California to resume his university studies. During this time Usinger collected specimens and data which resulted in the publication "The

[1] This manuscript has been reviewed by C. E. Pemberton, E. H. Bryan, Jr., and D. E. Hardy. Their comments and suggestions have been gratefully received, although the authors bear the responsibility of all conclusions reached.

genus Nysius and its allies in the Hawaiian Islands" which is today regarded as a classic study of insular evolution.

In 1936 the Hawaiian Sugar Planters' Association sent Usinger and Swezey on a four-month trip to survey the insects of Guam. A rich collection resulted and many of the specimens are in the Museum. Research on the material has been published by the Museum in two volumes (Bull's. 172, 189).

In the pre-war period, there were other smaller expeditions to the Pacific, including Samoa, Fiji, and Micronesia. Funds donated by C. M. Cooke, Jr. and Henry J. Lapham supported some of these trips. Much of the field work was done by Zimmerman. Also, Y. Kondo, then assistant in malacology, and Z. Ono of Japan, collected insects on the Micronesian expedition.

From Pacific outposts during World War II, many armed forces personnel made insect collections in their free time. Much of the material has generously been given to Bishop Museum.

CURRENT PROGRAM

Insects of Micronesia. From 1947 to 1953 the Office of Naval Research financed an extensive undertaking sponsored by the Pacific Science Board dealing with entomological, invertebrate, and atoll investigations of the Pacific. This resulted in the project entitled "Insects of Micronesia" which laid the groundwork for comprehensive studies of the arthropod fauna of Micronesia through the great amount of field work done by the ten entomologists sent to Micronesia during the six year period of the program. All the collectors were well qualified and recognized authorities in their fields. The ten were: P. A. Adams, R. M. Bohart, J. L. Gressitt, and R. L. Usinger, University of California; R. W. Potts, California Dept. of Agriculture; J. F. Gates Clarke, U. S. Dept. of Agriculture; H. S. Dybas, Chicago Natural History Museum; R. J. Goss, Museum of Comparative Zoology; N. L. H. Krauss, Territory of Hawaii Board of Agric. and Forestry; I. LaRivers, University of Nevada. J. W. Beardsley also did much field work for the project while employed by the Trust Territory. From this came nearly 400,000 specimens, about half of which will be deposited at the Bishop Museum, the bulk of the remainder going to the U. S. National Museum.

Major Expansion. In 1953 the Insects of Micronesia project was taken over by the Museum in the first step of major expansion in the entomological program. J. L. Gressitt had been employed in 1952 by the Pacific Science Board to reorganize and direct the program and in 1953 he became entomologist on the Museum staff, with the backing of H. J. Coolidge (Pacific Science Board), C. E. Pemberton (Pacific Science Board and Pacific Science Council), R. L. Usinger (University of California), and Alexander Spoehr (Director, Bishop Museum). The program developed with support from the Office of Naval Research, Pacific Science Board, Bishop Museum, and National Science Foundation. Bishop Museum began publication of the Micronesian studies in 1954, with financial help from the National Science Foundation. The project has involved research on the insect fauna of Micronesia by 130 collaborating specialists at some 50 institutions.

Zoogeography and Evolution of Pacific Insects. As a result of the Micronesian studies it became apparent that in order adequately to study the insects of the oceanic islands, it was essential to have available comparative collections from the adjacent major islands and continental areas, specifically the Southwest Pacific islands and Southeast Asia. It was also felt that these were the probable source areas of the oceanic fauna, the origin of which could not be determined without better knowledge of the insect faunas of the Southwest Pacific and Southeast Asia. Thus a program of field work for the "source areas" was gradually developed.

J. L. Gressitt carried out the first field work for Bishop Museum in the source areas in New Guinea and New Britain in 1955, on a Guggenheim Fellowship. At that time, application was made to the National Science Foundation for backing for field work in the source areas, and this commenced in 1956, when J. L. Gressitt and E. J. Ford, Jr. (U. S. Department of Agriculture, Plant Quarantine Branch, Honolulu) each spent several months in the New Guinea-Bismarcks-Solomons-Northeast Australia areas. Late in 1956 W. W. Brandt of Australia was employed for a long period of field work in the New Guinea area, jointly for Bishop Museum and the Australian government, with the Lepidoptera (Brandt's specialty) going to Canberra, and the other insects to Bishop Museum. During 1957 the program was further expanded by an increase in the National Science Foundation backing and the granting of funds for work on insects of public health importance from the National Institutes of Health. In 1957, seven entomologists participated in the field work, J. L. Gressitt collecting in New Hebrides, Solomons, New Guinea, Borneo, and Thailand, D. E. Hardy (University of Hawaii) in New Guinea, John Smart (Cambridge University) in New Guinea and Bismarcks, W. W. Brandt in Papua, C. W. Sabrosky (U. S. Department of Agriculture) in the Western Carolines, T. C. Maa (Chinese Quarantine Service) in Taiwan, and N. L. H. Krauss (Territory of Hawaii Board of Agriculture and Forestry), on official business in Guam, and at his own expense in the Gilbert Islands. During 1951, 1955, 1956, Krauss also collected at his own expense in Fiji, Samoa, Tahiti and Tonga.

In 1958, work is being carried on in Thailand, Malaya and British Borneo by T. C. Maa, in New Guinea by W. W. Brandt, in New Guinea and the Bismarcks by Elizabeth Marks (University of Queensland), in the Bonin Islands by F. M. Snyder (U. S. Army Chemical Laboratory), in New Caledonia and New Hebrides by Borys Malkin (University of Minnesota), in New Caledonia by Charles R. Joyce (U. S. Quarantine Station, U. S. Public Health Service, Honolulu), in the southern Philippines by H. E. Milliron, in British Borneo and Malaya by L. W. Quate, and in Thailand, Sarawak and New Guinea by J. L. Gressitt.

In 1957 a program was initiated for the trapping of airborne insects from ships crossing the Pacific. Traps are carried on Military Sea Transport Service ships operating out of Pearl Harbor to Manila and San Francisco. In 1958 the cooperation of the U. S. Coast Guard permitted placing of traps on weather observation ships between Hawaii and Japan. The purpose of the program is to study the natural dispersal of insects across the Pacific Ocean. It is hoped that the program can be amplified at a later date by having an entomologist spending part of his time travelling on the ships and by trapping from airplanes over the Pacific.

Staff: In amplifying the overall program, Dr. L. W. Quate, formerly Associate Professor of Entomology at the University of Nebraska, was added to the staff of Bishop Museum, primarily to work on Diptera of public health importance. Also, Dr. Carl Yoshimoto, Mr. T. C. Maa, Mr. Shinsaku Kimoto, Miss Carole J. Worthington, and others became associated with the program on a temporary basis. Miss Setsuko Nakata is in charge of the processing of the collections and Mrs. Dorothy Rainwater does illustrating, besides working on exhibits. Mrs. Nancy Blumenstock joined as half-time editor. Dr. Yoshimoto is concerned with the trapping of airborne insects, the sorting of collections, and taxonomic studies of Hymenoptera. Mr. Kimoto, Bishop Museum Fellow from the Kyushu University and Miss Worthington of San Jose, California mount and sort insects, as do other temporary assistants. Miss Amy Suehiro curates the Hawaiian collection and works on card-files of Pacific insects.

Collection: From this extensive field work, the Museum collection is growing rapidly. During 1956 and 1957, the collection nearly doubled in size, to exceed one million specimens. The increase during 1958 and 1959 will be even greater, with Mr. Maa on a full-time field basis, and addi-

tional specialists going into the field. The Museum will probably then have the world's largest collection from the Pacific Islands in general, and from Micronesia, Polynesia, New Guinea (except Lepidoptera) and N. Borneo in particular.

Identification Service: It is felt that Honolulu is a logical place for concentration of work on insects of the Pacific islands because (1). the Museum has a strategic geographical position and (2). because the insects of the Pacific area are of particular concern to the many Hawaiian entomologists, including those in quarantine work and biological control. For the study of many groups of insects, the Museum sends material to specialists actively working thereon, to mutual advantage. With additional funds for publication, we hope to publish many more monographs on Pacific insects to supplement those already published by the Museum, thereby aiding in the solution of identification problems. There is need for a Pacific Island insect identification center, and Honolulu is a logical location for it.

Publication: The "Insects of Micronesia" series has in less than four years made considerable progress, with 26 issues (31 articles covering 1,267 pages) published and more in press. Forty-seven out of a probable total of over 150 manuscripts have been received. It is expected that the completed series may include some 6,000 printed pages, dealing with about 5,000 species, many of which are new to science. The present field work concentration on the New Guinea and Borneo areas will result in collections exceeding those from Micronesia many-fold. This will provide material, also with a very high percentage of new species, for very extensive monographs. For example, the field work by Dr. Smart in New Guinea provided specimens of Simuliidae (black flies, buffalo gnats) which exceeded the previously known New Guinea fauna six-fold. Much of it is associated with immature stages. One of the first New Guinea manuscripts received, from Dr. Tokunaga on New Guinea *Culicoides*, doubles the known fauna, though based on only part of Gressitt's 1955 light trapping.

External Assistance: The rapid growth of the Entomology Department at Bishop Museum has been aided greatly by establishing a program of wide collaboration with other institutions, besides the invaluable support from the National Science Foundation and the National Institutes of Health. Sending well known specialists to distant places in the western Pacific has been possible partly through grants obtained from other sources by some of the participants to help meet travel costs, by institutions or government organizations cooperating in giving official leave with pay, by some individuals contributing their own vacation time, and by the U. S. Military Air Transport Service providing transportation from the Pacific coast or Hawaii to destinations in the western Pacific. The Pacific Science Board, the Office of Naval Research, and the U. S. Coast Guard have also extended generous aid and cooperation.

Conclusion: With nearly a quarter of a million specimens being received annually, the problems of storage space for the collections and necessary aid in curating are understandably acute. Increased financing on a more permanent basis is essential if the overall program is to continue and develop to the logical conclusion of extensive reference collections of identified specimens with duplicates deposited in other key museums, and continued publication of basic monographs.

Results of substantial value can be expected from the current activities. Accumulation of accurately identified specimens by competent specialists in several convenient locations, more precise data on the ecological requirements of the arthropods involved, and a more complete picture of the distribution of the organisms will be of importance in the study of the many problems of the Pacific area confronting workers in agriculture and public health. Quarantine officials and Army, Navy, and Public Health Service entomologists have already found the "*Insects of Micronesia*" publications of considerable use.

Insects have been the most successful animals in colonizing the widely scattered Pacific Islands and have developed a rich and varied fauna there. Studies of this fauna in the multiplicity of islands of different conditions of age, isolation, size, and ecological environments are invaluable to a clearer understanding of dispersal to, establishment on, and evolution within the islands.

Aside from the immediate goals of the entomological projects at the Bishop Museum, the Museum's staff has borne in mind the question of how scientific research can be most effectively organized. The present program is one answer to this question, which has been asked by scientists for many years. The program can be described as an experiment in collaborative research. It is a large program and involves many scientists. Each has considerable freedom in conducting his own work, both in the field and laboratory. By itself, the small entomological staff of the Bishop Museum is able to do but a part of the work and only through the unselfish cooperation of a large band of researchers can the desired goals be reached. The support of basic research through temporary financial grants from various foundations and agencies makes it possible for the Museum to continue to play its role in orienting the direction of the program and supplying the impetus for its continuation. Permanent sources of income are insufficient to support the large scale activities being carried on. Thus, the undertaking differs from that of many research centers in the United States today with more tightly programmed projects and staffs of a size capable of doing all or nearly all the research with the framework of one organization. The final appraisal of the Museum's program will have to determine if this method can produce fruitful results in modern research.

L. H. ELLIOTT PRESIDENT of UNIVERSITY of MAINE

Dr. Lloyd Hartman Elliott was installed as the ninth president of the University of Maine in impressive and dignified academic ceremonies at Orono, October 24, 1958 before some 600 delegates from colleges, universities, learned societies, educational associations, and foundations well over the Nation along with the faculty and administrative officers of the University. The undersigned represented the Entomological Society of America at the request of the Executive Secretary.

Dr. Elliott in accepting the post warned that in spite of the present demand for higher scientific training, education should not slight the humanities and moral leadership. He pledged his full capabilities to the position and the resources of the University to providing the highest quality education of which each student is capable. Means should be found of providing all capable youth a higher education. Both Dr. Elliott and Dr. Julius Bixler, President of Colby College, Waterville, in the opening address stressed that students must experience discipline and hardship to attain full maturity and goals of which they are capable. There is no place for the modern trend toward the soft life.

Following the academic exercises an inaugural luncheon for all delegates, guests, and wives was held by the University and finally a reception for the President and Mrs. Elliott.

Dr. Elliott started his duties on July 1 replacing the retired President since 1934, Dr. Arthur A. Hauck. Dr. Elliott is 40 and had been assistant to the President of Cornell University.

ROBLEY W. NASH
Augusta, Maine

Speculation and Research[1]

By Raymond C. Osburn[2]

When I was first approached about addressing this Institute it was suggested that I talk on the "philosophy of research". Knowing nothing about philosophy and very little about research, I tried to dodge, but my visitor wouldn't have it that way. Finally I compromised with him on the title "Speculation and Research". I would not have you think either that I consider myself a connoisseur on the subject of speculation, but my idea was that if I should fall down on the topic of research there would be no end of speculation to which I might resort.

As one looks back on the history of science it becomes evident that there has never been any dearth of that fascinating material. Also if one only looks about him today and consults the contents of our multitude of scientific journals, he is brought to realize that speculation is by no means one of the lost arts. If he goes outside of the strict field of science, he will find scarcely anything but speculation, that is to say, attempts to fix upon the correct answer in the presence of insufficient or conflicting data.

Before going any further I would like to make a plea for better understanding of this term speculation. I fear that most persons think of it in terms of "Wall Street" gambling. It has been dragged into bad company, but that is no fault of the word. As a matter of fact, it represents a certain stage in all our thinking, the elusive stage when we are "Sparring for an opening". Of course lots of mental exercise never goes beyond this point as far as results are concerned, and might just as well not be done. Speculation as an end in itself is about as futile as twiddling one's thumbs, but as a preliminary stage to some real work it is a necessary and highly useful procedure. The research scientist just could not get along without it, or perhaps I had better say it is a *sine qua non*, and show off my Latin.

It might be well at this point to introduce a few well-selected definitions, so that we may know what we are talking about, and besides good series of definitions adds *tone* to a scientific address. On the other hand, clear definitions spoil a lot of good arguments. If our honorable opponents in the discussion of matters philosophical, religious, political, and perhaps even scientific, would only just agree to our definitions, there might be no room for argument and we could settle all of our difficulties in no time at all.

But of course it is much easier to argue than to define and in polemics—the gentle art of verbal controversy—it appears to be necessary often to dodge the issue of a strict definition if there is to be a controversy. Political ballyhooers seemingly act on this with malice aforethought; religious throwbacks apparently do it instinctively; and some great orators do the same thing either because they are ignorant or illogical, or because they would make their point even at the cost of exactitude. If such men are honest, and they may be, there is something very obviously wrong with their thinking machinery.

The scientist is not interested in "making a case" by presenting the *argumentum ad hominem*, but only in knowing the truth, and therefore it behooves him to be very meticulous about his definitions.

I think I can best elucidate by an illustration "right down our own alley". Some of you may recall that Mr. William Jennings Bryan gave his remarkable outburst against evolution a few years ago. I would not go to hear him, as I had already heard him speak on political questions and knew something of his methods, but Mrs. Osburn was curious to hear him. She came home so mad that she was all "riled up" for a week and hasn't quite recovered even yet from the shock to her logic.

In the course of his harangue Mr. Bryan attempted a few definitions of his own peculiar type. He stated in connection with the Darwinian theory, that "a theory is a hypothesis", which may be partly true; that a "hypothesis is only a guess", which is partly true, and wound up by quoting the old saw that "one man's guess is as good as another's", which isn't necessarily true at all. Shades of Samuel Johnson, Noah Webster, and all of the other old lexicographers!

Huxley pointed out the difference between the average man's guess and the scientists' guess or speculation by saying that the guess of the scientists is a very serious matter after weighing carefully all the possible data and is no off-hand proposition.

If Bryan had consulted a good dictionary he would not have fallen into such an error (or maybe he would just the same). Being more careful of my statements, probably as a result of scientific training, I have consulted my old friend, Webster's Unabridged, and find the following definitions:

1. *Guess:* "An opinion formed without any sure grounds of inference."

Do scientists ever guess? Certainly they do, just like other people, though hardly with the same recklessness. But unlike most other people, they are, or should be, trained to appreciate the fact that their guesses have no particular importance, but are merely possible leads to be followed up in order to determine whether there may be anything in them.

The guesses of the scientist are merely the first tentative steps in a long, careful series that may lead nowhere or somewhere. In any event, the scientist does not "rest his case" on the guess, for he knows when he is guessing and realizes fully the invalidity of reasoning from such a slight basis.

Sweeping generalizations are made constantly without any proper basis by people who never thought of checking up anything to get at the real truth. I find an excellent example in Ted Robinson's "Philosopher of Folly's Column" in the *Cleveland Plain Dealer*, and as I like to be up to date in my references I quote it:

Progress

"The people of antiquity were cradled in iniquity
And nurtured in the very depths of ethical obliquity;
But oh, their sweet posterity are famed for their severity
Of morals, faith and principles, uprightness and sincerity!
The earliest community at every opportunity
Could kill and steal and brew illegal liquor with impunity;
But nowadays society is noted for its peity—
We only kill a little, and we know no insobriety.
Grandad was a monstrosity of primitive ferocity
Whose ordinary conduct was a positive atrocity;
But our immense majority respects all high authority—
From whom did we inherit such a great superiority?

"(Some say that the rapacity, pugnacity, salacity,
Are ours, while all our ancestors had culture and sagacity;
Thus showing the futility and positive senility
Of generalizations made with such a glib facility!)"

The unscientific mind is not bothered by any checking-up process and finds belief and the acceptance of loose guesses an easy matter. Especially this seems to be the case with old beliefs. Just give a belief the advantage of age and it seems to assume a sacred character in the minds of many people, whether it ever had any basis in fact or not, and many of them do not.

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[1] Read before The Plant Institute, November 25, 1935.
[2] Died August 6, 1955.

Professor Henry C. McComas of the Department of Psychology of Johns Hopkins University in a recent book, "Ghosts I have Talked With" outlines clearly the attitude of mind of the gullible and believing public, of those who find it easier to accept the ancient, traditional, often mystical and anti-scientific explanations of natural phenomena. He says·

"The primitive mind makes its appearance in our present day, by following *simple forms of reasoning.* Among men who have not had the benefit of a thorough education and who possess simply an average intelligence it is common to find them arguing from one or two instances to a general conclusion. . . . Fishermen are full of stout beliefs that certain fish do certain things and defend their statements with one or two examples. Every political campaign supplies us with politicians who clinch their arguments by citing a single case chosen from history. Obviously this sort of thinking has nothing in common with science. Since the time of Roger Bacon we have had the laws of induction clearly before us. It is one of the characteristics of modern science that no general conclusion can be drawn while contradictory facts may be collected. One of the best illustrations of scientific attack upon a problem is afforded by Jenner before the time that our modern science had established its methods. Jenner cited so many cases where cow-pox made its victims immune to small-pox that neither the skeptical physicians nor the hostile churches could deny him. Indeed if any one thing characterizes our modern science it is this insistence upon a large number of observations to establish any generalization. . . . It is so easy to draw conclusions from *simple analogies* that we find everyone doing it. . . . Before we began to study the chemical action of drugs upon the tissues of the body our ancestors hit upon some analogies. If there was trouble with the blood they used bloodroot on account of its red juice; if the liver made trouble they used liverwort as it has a leaf shaped like the liver; . . . bear grease coming from an animal with thick hair was entrusted with the duty of growing hair on bald heads."

One might extend indefinitely these illustrations of the ancient medical principle of "*similia similibus curantur*"—snake oil from that flexible animal as a cure for the "rheumatiz", "the hair of the dog is good for the bite", etc., through pages of citations of just such erroneous conclusions formed erroneously from analogies or based upon one or two coincidences.

I am minded to cite just one case resting upon a single coincidence. A good many years ago I was interested in seeing what the ancients knew about the little fish called the sea-horse (*Hippocampus*). In Dioscorides and Aelianus (first and second centuries A.D.) I found that the sea-horse, either macerated or incinerated and administered in wine or oil, was a sovereign remedy for rabies. This idea was accepted all down the centuries until the development of modern medicine. It rested upon the following datum; A fisherman was out with his two sons, but was unable to take anything but a sea-horse. (These fishes have a habit of hanging on to weeds, etc. with their prehensile tails and are often taken on fish nets.) On land, one of the boys was bitten by a dog and immediately went into convulsions. As the story goes, the sympathetic neighbors stood around and suggested various remedies, but none of them worked. (Neighbors haven't changed much since the ancient times.) At last the father happened to think that perhaps the sea-horse was meant as a sign or omen, so he mashed it up and administered it to the boy in wine and he immediately recovered. I presume all he needed was a "shot of hooch" for a little stimulation. At any rate the boy could not have had rabies in a few minutes after he was bitten and of course neither the wine or the sea-horse would have done him any good if he had. Yet for some 16 or 17 centuries this remedy was in the standard works on therapeutics—and we poke fun at the Chinese remedies!

I think we may take it as a rule that the older and wider spread the "old sayings" are, the more we should subject them to close scrutiny. A great many of them which people in these supposedly enlightened United States readily accept have been brought from other countries and other conditions, where they might have applied. Many of our so-called weather signs are introductions from the local incipient meteorology of England, Germany, etc., and have no more application here and today than the laws of the Medes and Persians. People solemnly knock on wood three times without even knowing that they are going through the ancient religious ceremony of touching the cross in the name of the Father, the Son, and the Holy Ghost. The superstition about killing a cat probably harks back to ancient times in Egypt where the cat was held as a sacred animal.

I noted in a recent magazine section of the *Columbus Sunday Dispatch* (November 24, 1935) that one Claudia de Lys claims to have "debunked" some 8000 of these old superstitions. Very good, Claudia, if you have done it. In fact, my dear Claudia, there are two "ifs". No. 1, whether you have done it or whether advancing knowledge of natural phenomena has not done it instead. No. 2, whether you or anyone else could do it for the great majority of the world at large, so fixed are they in their traditional beliefs and so little basis have they for anything else but belief. It does not seem to make much difference how simple such a belief may be, so long as it is traditional. In fact the simpler it is, the better, for then the simple mind may grasp it. The facts and explanations of science must forever remain a mystery to most of mankind, who will continue to accept crude, unscientific guesses based upon coincidences and analogies.

2. *Speculate:* "To pursue inquiries and form conjectures on any object in the mind, especially *a priori* and without experiment."

Do scientists speculate? Surely they do, and I wonder where any scientist would get without doing it constantly in his research. He takes his guess, or several guesses for that matter, mills the question over in his mind, tries it this way and that with whatever data are at hand, or in other words speculates in *a priori* fashion to see if he can possibly catch a glimpse of light ahead, to determine in what direction may lie the best opening for experimentation. But this is all in *preparation* for putting the case to the *experimental* test. If the results are unconvincing, he drops that line and sets up another line of attack. But the scientist knows perfectly well what he is doing and his speculation is just so much mental trial and error behavior, to be discarded if it should lead nowhere, to be expanded by research into a theory or a truth if found to be correct.

I realize that there are occasional scientific men who may make an assumption and later argue from that as though it were a proven fact, but insofar as they do this they are altogether unscientific. It is probably too much to hope for that all scientists should be as careful in expressing themselves as are the members of this audience.

3. *Hypothesis:* "A statement of fact or theory which without itself being proved, is taken for granted as a premise from which to test or discover an assured conclusion: a logical conclusion."

A logical conclusion is not necessarily true, of course. The logic may be incontrovertible, but may be based upon false assumptions. In this it differs sharply from scientific proof.

A hypothesis is just what it claims to be, a *hypo* thesis, and may be either a fact or an assumption upon which to develop a thesis. As an example we are all familiar with the Nebular Theory of Laplace, based upon the hypothesis that the space now occupied by the solar system was once filled with highly heated gaseous nebular matter of which our present sun and planets are the consolidated residue, etc. The later discovery of contradictory facts resulted in discarding the nebular theory as a scientific explanation of the origin of the solar system, though for many years it appeared to meet all the necessary conditions. As someone

has said, "A beautiful theory may be slaughtered by an ugly fact."

4. *Theory:* "A mental plan or scheme framed to agree with the observed facts and designed as a rational explanation of them."

Compare this definition, if you will, with that of a guess —"an opinion formed without any sure grounds of inference", and Mr. Bryan's logic reminds one of the old joke about a loaf of bread being the mother of the steam engine, for—bread is a necessity; necessity is the mother of invention; a steam engine is an invention. Q.E.D.

Perhaps I should go one step further and define "Natural Law", which is the desideratum of all our research and scientific cogitating. If you have all read Pearson's "Grammar of Science" it may not be necessary, but on suspicion that some of you have not, I may point out that a natural law is the product of a rational analysis of facts, a scientific statement, based on research, of the way things behave in nature, in other words a general truth concerning nature.

"The law of gravitation is a brief description of *how* every particle of matter in the universe is altering its motion with reference to every other particle. It does not tell us why particles thus move; it does not tell us why the earth describes a certain curve around the sun. It simply resumes, in a few brief words, the relationships observed between a vast range of phenomena. It economizes thought by stating in conceptual shorthand that routine of our perceptions which forms for us the universe of gravitating matter.

"We have in the law of gravitation an excellent example of a scientific law. We see in its evolution the continual struggle of the human mind to reach a more and more comprehensive and exact formula. . . . Men study a range of facts . . . they classify and analyze, they discover relationships and sequences, and then they describe in the simplest possible terms the widest possible range of phenomena."

I have placed my definitions in the order in which the scientific mind appears to work in arriving at generalizations. To summarize the process it appears something like this:

1. A scientific guess, based on a few facts which seem to point in some direction, which may give rise to
2. *speculation* concerning this guess, turning it around and about, philosophizing over its possibilities, selecting what appears to be the most probable lead and setting this up as a
3. *hypothesis* to be tested in all possible ways by further research and experimentation to prove its truth or falsity. If it appears to be true, the hypothesis is expended into a
4. *theory*, which, when sufficient data (none conflicting) are accumulated, may assume the dignity of a *natural law* or general truth of nature.

Please don't imagine that I, or anyone else, ever set himself down and said to himself, "Now I'll make a scientific guess and then speculate about it, set up my hypothesis and then I'll go ahead and experiment and research and investigate and work out a theory!"

No, we never take any thought of the procedure indicated. That is just a statement of the way we have done the thing. It is something like the old formal logic which never helped a man to form a syllogism, but which showed him how he had thought after he had done his thinking.

As a matter of fact these processes are usually all mixed up. Some fact hits us in the eye and we get to wondering about it. We look about and find another fact. The beauty about *two things* is that you can *always line them up*, although they may not point anywhere in particular. A few more facts lined up however, may provide one with a real "hunch". Our research usually begins right at the time our interest is aroused by the first fact and we go on guessing, experimenting, speculating, researching, thinking, testing, theorizing, investigating, and generally carrying on all the processes at once. Not all minds work the same way, of course. There are some, perhaps more philosophically minded, who will have a whole extended theory laid out from the time the first fact strikes their attention. Perhaps they cannot help it, but this method is likely to be unsafe, as such persons are too apt to set up a theory and then try to find facts to support it.

On the other hand, the scientist who is not at the same time a dreamer is not likely to contribute greatly to knowledge. He may be serviceable as a fact collector, but facts are not science, they are merely the raw material out of which science may be constructed. A pile of bricks may have no special meaning, but properly selected and with an intermedium of plaster they may be molded into a university hall or a scientific laboratory. Similarly, masses of facts amount to little as such, but properly selected and judicially fitted together with the right kind of cogitation, they may be erected into magnificent and enduring theories and natural laws.

It is the proper mixture of research or fact-finding and speculation or philosophizing that yields results. Professor E. B. Wilson of Columbia University, once said to me that "Every scientist must be something of a philosopher and every philosopher ought to be a good deal of a scientist." Certainly the philosopher will arrive at nothing but philosophy if his only tools are the old armchair and a few assumptions.

The word "speculate" comes from the Latin root "*specio*", meaning "I see", but speculation on the research road unfortunately does not see very far at a time, as a rule. However, if it helps one to see a possible way as far as the next turn or obstruction, that is worth while. When the investigator arrives at the end of his first look, he must put on his speculating glasses and look again. The solution of a research problem reminds one of the information given in some hill countries concerning a certain location as being so many "looks ahead", only in the research problem no one knows how many looks ahead the end may be, since no one has traversed that region before.

Furthermore, the research road may send off side branches or ramify into any number of paths to new research problems. One of our stock questions is the final examination for the doctorate is whether the candidate has found any side issues or further problems that his research has opened up. I have known a bright student to become quite incoherent in trying to enumerate all at once the suggestions for further work that have occurred to him. Also I have known more than one man of good research caliber who could seldom bring himself to finish a problem because of the atttraction presented by the multiplicity of side issues. In order to get results one must stick to the main road. Like sight-seeing in the mountains, one must go on toward his destination and say to all the attractive by-paths, "I'll come back and see you some other time."

Another interesting thing about science is the fact that no natural law or generalization ever becomes so complete that further work may not be done on it. We look upon such matters as gravity, the periodic law and evolution as being thoroughly established, no longer merely theories but great laws of nature. Yet most, if not all, of us have seen the law of gravity extended by the conception of relativity, the gaps in the periodic law almost completely filled in and its meaning expanded, and new additions, interpretations and explanations to the method of evolution.

The development of any important principle or major natural law is a matter of slow development, of much speculation, of painstaking fact gathering. In some cases centuries, even millennia, have elapsed between the first loose speculations and any definite results.

The old Greek philosophers, several centuries B.C., caught a glimpse of the great principle of evolution, both organic and inorganic. Augustine, in the 4th century A.D., expressed a belief in a perfecting principle in nature which brought about change and improvement in organisms

lower than man. De Vinci, more than a millienium later, speculated to himself in his notebooks concerning change in nature. In the 18th century, Buffon, in the face of the established dogma of a tyrannical church, ventured to express the results of certain observations along the same line. In 1794, Erasmus Darwin, in the freer atmosphere of Protestant England, could say in his *Zoonomia*, "As the habitable parts of the earth have been, and continue to be, perpetually increasing by the production of sea shells and corallines, and by the recrements of other animals and vegetables; so from the beginning of the existence of the terraqueous globe, the animals which inhabit it have constantly improved, and are still in a state of progressive improvement."

Fifteen years later, Lamarck (*Philosophie Zoologique*, 1809) made the first extended effort to express the law of organic evolution, with an attempted analysis of the factors involved. But the world of science and philosophy was not yet ready for such an overwhelming advance. Moreover, some of Lamarck's speculations were without proof, as has since been shown, and another half century had to elapse before Darwin, in 1859, brought forth his *Origin of Species*, which was to upset and then clarify the preconceived notions of the scientific, philosophic, and theological worlds. But a tremendous amount of data, comparatively, had been accumulated by 1859 in biology, geology, chemistry, physics, and astronomy, enabling Charles Darwin to see much farther into the problem than any of his predecessors.

As might be expected, some of Darwin's speculations have been shown to have been unfounded, but these have not affected the general truth of organic evolution. Since that time the best thought of the world has been centered upon this question, both *pro* and *con*, every scrap of possible evidence has been scanned again and again, research and experiment have contributed an enormous amount of additional data. It is safe to say that more speculation and research have been centered upon this question than on any other law of nature, with the result that the law is as firmly established as any other in the minds of people capable of forming an opinion.

Similarly, the law of gravity had its origin among the first watchers of the movements of the stars. The Greek philosophers again speculated over this matter; Ptolemy in the first century A.D. contributed the first system, mostly erroneous as we now know; Copernicus, in the 16th century contributed to the theory the first correct statement of the motions of the solar system; and Tycho Brahe, Galileo, and Keppler all added to the data which made possible the great work of Newton.

I have heard it stated that Newton was jolted into the consideration of the problem of gravity by an apple falling on his head. If so, that apple made up for a lot of the harm another classical apple is supposed to have done. I venture to say, however, that it was not the first time that an apple had done the little trick of "beaning" someone, and the chief difference lay in the fact that it was Sir Isaac who got bumped this time and he had all the necessary mental machinery as well as the work of his predecessors to aid him, and the result was a great law of nature.

Only once in a while does a great principle appear to pop out at once from nowhere and become established from the start. Such an example may be found in the cell theory. From the time of Harvey and Malpighi, scientists have been looking at living matter with the aid of their lenses. In 1801 Bichat founded the subject of histology. Various other great biologists came and went and looked right past the cells at the tissues. It was not until 1838–9 that Schleiden and Schwann noted the very evident fact that all of the tissues of both animals and plants are made up of unit masses of living matter. The thing was so perfectly evident that every other biologist must have felt ashamed of himself for not having noticed it.

I find it difficult to define the term "research", and even more so to indicate its scope. Webster indicates that it is "diligent, protracted investigation, especially for the purpose of adding to knowledge." I presume this is about as close as we can arrive at a definition safely. It appears to exclude the dilettante by the terms "diligent" and "protracted." A man must really have some purpose in mind if his investigations are diligent and protracted. Furthermore, the latter term seems to imply what some writers have insisted on, namely that the real emphasis of research should be on the first syllable, in other words, to search again and again. The word coming to us from the Latin through the French originally meant to search again. Real research cannot be done without some basic knowledge of subject matter, for without this I cannot conceive how one could have any interest or any insight into a problem. One could multiply cases of lost energy and time spent in useless experimentation because of the lack of basic information. When I was a little chap I heard about perpetual motion, and it seemed to me at that time to be a pretty good idea if one could only make it work. I do not recall worrying very much about the matter, but I must have given it some consideration because one night I had a very vivid dream in which I worked out the whole machine. It consisted of a long tube pivoted at the middle, with a very resilient spring in each end, and a heavy ball. The ball dropped into one end would lower that end, but on striking the spring would be thrown back into the other end and lower it, and so on. Even at that age I had sense enough to see that it would run down before it got started. However, men with no knowledge of the laws of physics are still "monkeying around" inventing perpetual motion machines which do not work.

Given a little information to start with and a lively imagination, together with what someone has called "divine curiosity", and problems for research spring up everywhere. Some 15 years ago I was out in field work on a survey for the Fish and Game Division and had as one of my assistants a young graduate student from another department. I presume we must have been saying something about research work, for he asked me "How does one go about it to find research problems?"

I cannot recall the exact conversation, but I do remember saying to him that if he were interested in any subject, no matter what it might be, and would look up the literature to find out all about it, he would very soon come to the limit of the recorded information. Right there a new problem for research began. He could advance no further with the aid of the literature and would be up against a blank wall, beyond which lay a *terra incognita*. If he wanted to know what lay beyond, he would have to devise some method of scaling that barrier to the unknown. There his basic prepartion, his speculative ability, and his ingenuity in devising methods would all come into play. Some barriers are insurmountable at present because we cannot find out how to get over or by them, so some problems must wait for solution at some future time when we hope we may know more about how to tackle them. However, there are countless workable problems, for *every line of thought and every field of interest has a research problem at the end of it.*

The earnestness of the young man impressed me and I could go to the same place along the north side of Buckeye Lake where we discussed this matter as we walked along. I have no doubt he could do the same for he reminded me some years later of the conversation and assured me that he had never found himself lacking in research problems. The gentleman mentioned is now a highly respected member of the Faculty and has a nice little growing international reputation of his own for research in his special field.

There is nothing new about research. Adam and Eve began it when they bit into the apple to see what would happen. It happened and they didn't like it, as with many others who have bit off more than they could properly chew. Nevertheless, it was investigation and it surely started something. Aristotle stressed observation as the only sure method of arriving at safe conclusions. Vesalius was a real investigator. So was Harvey. Redi, on the first

page of his classical *Experiments on the generation of insects* (1688), in which he overthrew the idea of spontaneous generation, quotes an Arab proverb thus: "Experiment adds to knowledge, credulity leads to error."

Redi, however, tells us but little about how he achieved his results and Harvey also gives us no clue as to his methods. The first to outline his method of work carefully was Spallanzani about 1780, and since that time it has been customary to indicate research procedure, for it is often as important to know how a man got his results as it is to know what was achieved.

As I define the term, and I think most scientists at any rate would agree with me, there can be no real research without preceding and accompanying speculation. It is possible, to be sure, to engage in mere fact-finding without bothering one's self about the meaning of the facts or the use to which they may be put, but this procedure, while it may be useful to others, can scarcely be called research.

On the other hand, pure speculation without investigation and experiment may lead one very far away from the truth. There have been striking examples in all branches of scientific thought, especially in the earlier stages when men were merely philosophizing over natural phenomena and before the methods of science had been so clearly established. A sample or two of this may suffice as "awful examples":

One of the most striking cases of biological speculation running riot is found in the efforts of the 17th and 18th century philosophers to explain the process of reproduction. About all that was known of this process until the latter part of the 17th century was that all animals come from eggs and that for some obscure reason it is necessary for the sexes to come together for reproduction.

Harvey, about 1630, expressed what was known at that time in his peculiar phraseology: "Although it be a known thing subscribed by all, that the fostus assumes its original and birth from the male and female, and consequently that the egge is produced by the cock and henne, and the chicken out of the egge, yet neither the schools of physicians nor Aristotle's discerning brain have disclosed how the cock and its seed doth mint and coin the chicken out of the egge." Then in the 1670's, the spermatozoon was discovered in the seminal fluid. This discovery had the misfortune to attract the attention of the philosophers rather than the investigators, and the problem of reproduction was soon obscured by the wildest speculations that were ever made even in a period rife with speculation. As a matter of fact, the discovery of the spermatozoon amounted to but little at the time, except to stir up a cyclone of words and a further array of baseless speculations.

At first the seminal *fluid* was thought to be the important material in exciting the development of the egg, and the spermatozoa merely parasites in it. But soon there grew up two adverse schools of speculation which, after the manner of the time, disputed violently about matters of which neither really knew anything and, as time and investigation have proved, both were wrong.

One of these schools, known as the "Ovists", argued that the egg is the essential thing in reproduction, that the sperm merely stimulates it into development, and that within the egg is laid down the structure of the organism in such form that all it has to do is to grow and unfold, just like a young plant in a seed—to *evolve* (in another sense from which the term evolution is now used). This supposed method of development was known as "preformation". Some of the "Ovists" went further and maintained that within the egg there is the similar and still smaller rudiment of that individual's offspring, etc., etc. Bonnet was the champion of this group of high-powered speculators, which apparently was not in the least bothered by the logical application of this notion, that by reversing the story, in the eggs of Mother Eve there must necessarily have been stored all the generations of humanity for all time, one generation within the other, so long as the world should last. This is a fine example of what is known as *reductio ad absurdum*". But there seemed to be no limit to the absurdities of speculation in this period, and furthermore, the preformation theory fit well into the theological doctrine of predestination which was current at the time

The opposed school, the "Spermatists" or "Animalculists" were no more free from wild absurdities than were the "Ovists". They maintained that the sperm was the important structure and that in the head of the sperm was the "homonculus" or offspring. Some of them even published illustrations, showing in detail the lineaments and the arms and legs rolled up. Their notion was that all that was necessary for one of these to develop was for it to get into an egg, where it would find the proper environemnt.

As late as 1794 the great Erasmus Darwin, grandfather of the immortal Charles and one of the ablest men of his time, in his *Zoonomia* argues the matter *pro* and *con* in his masterly fashion and comes to the conclusion that the sperm contains the germ of life and is, as he says, "a part of the father", though he does not claim that any organized parts can be seen. He accounts for the maternal influence on heredity by supposing that the egg, which is the environment of the sperm for further development, contains nutritive matter which leaves its mark on the offspring. The egg, he believed, is made up of nutritive particles derived from all parts of the body of the mother, but contains no germ of life. The sperm, on the other hand, is a living portion of the father, without stored nutriment, but containing all his "propensities" and "appetencies." The sperm finds in the egg its proper environment for growth and, as it absorbs the already prepared nutriment in the egg, its development is affected by the amount and kind of the various maternal substances stored therein. So the embryo comes to partake of the nature of both parents, as shown especially in the case of mules, mulattoes, etc. He even extended the speculation to cover teratological conditions—the lack of some portion of the egg causing failure to develop, as in harelip, split palate, etc., and an overplus of some maternal substance causing reduplication, such as extra fingers, limbs, etc. What a waste of good ink!

But the period of Erasmus Darwin is excessively remote in the accumulation of data and of scientific thought, for all the branches of natural science were at that time, only 140 years ago, only in their infancy. These samples of biological speculation show what could be done and was done in the "hey-day" of the speculators. All that ever resulted from it were headaches and hot air.

But speculation is not all of this loose type. I have already referred to the essential type which precedes and accompanies investigation. There is still a third type, which for want of a name we may call "appended speculation"—the type that may properly be attached at the end of a scientific paper in calling attention to further possibilities for research, or in suggesting possible explanations for which the data at hand are insufficient yet as proof.

Some scientists are averse to throwing out speculations. There may be two reasons for this. In some cases they may fear that someone else will beat them to the solution, but I believe this group is not as numerous as it once was when many men carefully guarded the secrets of their research until ready to publish. For one thing, it is not now considered ethical to "swipe" another man's problem on which he is known to be actively working. Moreover, he who filches from me my "good" problem doesn't leave me so "poor" after all, for if I am worthy of one good problem I will have a lot more "on tap." Probably a better reason for reticence in venting one's speculations in this way is the natural reluctance of the average scientist to being found in the wrong, and anyone who throws out such suggestions lays himself open to contradiction when the facts are assembled.

Be this as it may, we do have some spendid examples of scientific speculators of this third type, who in addition to great contributions of their own, have stimulated a much greater production by others as a result of the suggestions

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which they have thrown out. Agassiz at Harvard, Brooks of Johns Hopkins, Cope at Pennsylvania, Wilson and especially Morgan at Columbia, H. F. Osborn at the American Museum, Whitman at Chicago are excellent examples. Perhaps the most striking case was that of Jacques Loeb, for many years at the Rockefeller Institute, who threw out speculations in all directions at the end of his papers. To be sure, there was a very high mortality rate when these ideas were tested, but at any rate they irritated other research men to investigate and discover the truth. The value, and sometimes the only value, of this type of speculation lies in the possibility that it may set someone to thinking and working.

Crabbed old Carlyle once gave pointed advice something like this: If you have something to produce, even if it be only the most infinitesimal sort of production, in God's name produce it!" Paraphrasing this I might say that if you have only an infinitesimal speculation to make, in God's name speculate it, for it may set someone thinking and investigating. If he proves you correct, he will probably quote you and "boost your stock"; if he finds you wrong he will most likely "tell the world" about it, but in the meantime we will learn the truth and that is what we are all after.

I hope you do not expect me to discuss the "spirit" of research, or to tell you off hand how to become better research workers. I do not know just what the spirit of research may be. I have always worked at what I call my research because I would rather do that than anything else in the world and I have more than an inkling that this is true of all who carry on investigation for any extended time. I have known younger men to carry out a problem set for them in completion of a higher degree and fail to do anything of importance in research afterward. Evidently they did not find it to their liking above all else, or in other words they lacked the spirit of research. I see nothing mysterious in the spirit of research—to my mind it is just as simple as I have stated it. We sometimes hear of a department or group having more of the spirit of research than some other, and that is easily explained on the ground that more people who like to do research have ben brou ght together.

I am quite aware that some persons can accomplish much more in research than others, and I am also quite sure that it is not merely because they like it better. There are all sorts of factors entering here—slow thinking, poor memory, lack of basic knowledge, limited ability in speculation, etc., etc. The genius is usually a man who has the proper preparation, keen speculative vision, and the physical energy for continued investigation, as well as the will to put the job across. He does not accomplish his work because it is the thing to do, nor because some one sets him at it, but because he prefers it to all else and because he cannot be happy unless he is reaching out all the time towards the unknown. In any but such a signified field he would be known as a research "fan."

It may be possible for some of you to improve your research output by broadening your basic preparation or by applying yourselves more diligently, but I cannot tell you how to quicken your imagination nor to increase your love for this type of work until you are wedded to it, "forsaking all others" and "until death do you part."

POSTDOCTORAL RESIDENT RESEARCH ASSOCIATESHIPS

The National Academy of Sciences-National Research Council has announced a program of Postdoctoral Resident Research Associateships to be offered for 1959–1960. The participating laboratories are the *National Bureau of Standards* (Boulder, Colorado and Washington, D. C.); the *Naval Ordnance Laboratory* (White Oak, Silver Spring, Maryland); the *Naval Research Laboratory* (Washington, D. C.), the *Navy Electronics Laboratory* (San Diego, California), and the *U. S. Army Chemical Corps Biological Warfare Laboratories* (Fort Detrick, Frederick, Maryland).

The *Air Research and Development Command* is also participating in this program at four Air Force installations. These associateships are tenable at Air Force Cambridge Research Center (Bedford, Massachusetts); Air Force Missile Development Center (Alamogordo, New Mexico); Rome Air Development Center (Rome, New York); and Wright Air Development Center (Dayton, Ohio). In addition, the *ARDC* is sponsoring a program of *Postdoctoral University Research Associateships* tenable at twenty-one universities in the United States.

The resident research associateships have been established to provide young scientists of unusual ability and promise an opportunity for advanced training in basic research in a variety of fields. Modern facilities are available in specified areas of the biological, physical, and mathematical sciences, and engineering. In addition to the above research in certain areas of psychology is available.

Applicants must be citizens of the United States. They also must produce evidence of training in one of the listed fields equivalent to that represented by the Ph.D. or Sc.D. degree and must have demonstrated superior ability for creative research. Remuneration for these associateships is from $5985 to $7510 a year subject to income tax.

Application materials may be secured by writing to the Fellowship Office, National Academy of Sciences-National Research Council, 2101 Constitution Avenue, N.W., Washington 25, D. C. In order to be considered for awards for 1959–1960, applications must be filed at the Fellowship Office on or before January 19, 1959. Awards will be announced about April 1, 1959 by the participating laboratories and research centers.

N.P.C.A. SILVER ANNIVERSARY

The Silver Anniversary meeting of the National Pest Control Association was held at the Statler Hilton Hotel in Washington, D. C. on October 20–23, 1958. Executive Secretary Ralph E. Heal, Technical Director Philip J. Spear, President J. C. Redd, and Secretary-Treasurer Paul J. McMahon, N.P.C.A. officers in charge of this meeting, are also members of the Entomological Society of America. We want to congratulate these officers and the N.P.C.A. on their excellent meeting.

Honorary members of the N.P.C.A. who were present and who are also members of the Entomological Society of America were as follows: F. C. Bishopp, A. I. Bourne, F. L. Campbell, J. J. Davis, A. S. Hoyt, E. F. Knipling, Arnold Mallis, W. E. McCauley, J. V. Osmun (who was also master of ceremonies at the banquet), R. C. Roark, R. E. Snyder, and H. L. Sweetman. Of the new Honorary Members elected Hamilton Laudani is also an ESA member.

The October 19–22, 1959 meeting of the N.P.C.A. will be held at the Buena Vista Hotel in Biloxi, Mississippi.

PROCEEDINGS OF THE SIXTH ANNUAL MEETING OF THE SOUTHWESTERN BRANCH, ENTOMOLOGICAL SOCIETY OF AMERICA

Houston, Texas, February 10–11, 1958

The Sixth Annual Meeting of the Southwestern Branch of the Entomological Society of America was held in Houston, Texas, February 10 and 11, 1958.

The meeting was called to order by Chairman C. R. Parencia. Following the invocation, Vice-Chairman R. R. Walton assumed the chair and presented the Honorable Lewis Cutrer, Mayor of Houston, in an Address of Welcome. Following the address, R. R. Walton introduced the Chairman, C. R. Parencia, who gave the Annual Address of the Chairman.

Following the appointment of committees, Dr. C. M. Meadows presented the report of the Southwestern Branch Member of the Governing Board.

The assembled group of 200 enjoyed remarks of the President of the Entomological Society of America, Dr. R. L. Metcalf, relative to the status and future plans of the national society.

The Invitational Address, "The Organic Phosphate Insecticides—An Agricultural Occupational Hazard," was presented by Dr. Charles S. Petty, Louisiana State University, School of Medicine.

Forty-Five submitted papers were presented during the meeting.

A dinner was held Monday night, February 10, followed by the awarding of door prizes, which, according to those present, was most enjoyable.

Chairman Parencia called for the following reports:

Treasurer's Report

3-1-57	Balance On Hand	$ 614.42
3-21-57	Registration Fee 1957 Convention	444.00
	TOTAL AMOUNT ON HAND	$1,058.42

Disbursements:

Printing & stationery (1957)	$232.85	
H. M. Armitage (Hotel bill, 1957 conv.)	21.00	
Printing, stationery, badges, etc, for 1958 meeting	328.82	
Branch Chairman—telephone expense (1957)	13.70	
	596.37	596.37
Balance February 5, 1958		$ 462.05

Respectfully submitted,

SHERMAN W. CLARK
Secretary-Treasurer

Report of the Auditing Committee

We, the auditing Committee of the Southwestern Branch, Entomological Society of America, have on this 10th day of February, 1958, examined the receipts and expenditures of the Secretary-Treasurer of the Branch and do hereby certify that said receipts and expenditures are in order.

Respectfully submitted,

K. D. ARBUTHNOT
GEORGE T. DAVIS, JR.
MANNING A. PRICE, *Chairman*

Report of the Nominating Committee

The *Nominating Committee* nominated officers for 1958 as follows:

Chairman, Paul Gregg
Vice-Chairman, Manning A. Price
Member Governing Board, Chas. M. Meadows
Secretary-Treasurer, H. E. Meadows

These officers were unanimously elected by vote of the members present.

Report of the Committee on Resolutions

WHEREAS, the members of the Southwestern Branch of the Entomological Society of America in its Sixth Annual Meeting have enjoyed the friendly hospitality of the City of Houston as personally extended by His Honor, Mayor Lewis Cutrer.

BE IT RESOLVED, that we convey to him our appreciation and thanks.

AND BE IT RESOLVED, that we thank Rod Morgan, Sales Manager of The Shamrock Hilton Hotel for the facilities provided which have contributed so much to our convenience and comfort.

AND BE IT RESOLVED, that the *Local Arrangements Committee* consisting of W. Kenneth Conwell and M. F. Wilkerson be highly commended for making our meeting so pleasant, profitable, and successful.

AND BE IT FURTHER RESOLVED, that we extend thanks to the *Program Committee* for assembling and organizing a well rounded program.

FINALLY BE IT RESOLVED, that copies of these resolutions be transmitted by the Secretary to the Committies and individuals who have carried out their responsibilities so effectively.

Respectfully submitted,

F. L. THOMAS
February 11, 1958 G. A. BIEBERDORF, *Chairman*

Dallas was selected as the meeting place for the 1959 convention. The consensus of opinion of the membership was that this was one of our most successful conventions.

Respectfully submitted,

H. E. MEADOWS, *Secretary-Treasurer*

RICHARD HEATHCOTE HEINDEL PRESIDENT of WAGNER LUTHERAN COLLEGE

Dr. Richard Heathcote Heindel was inaugurated President of Wagner Lutheran College, Staten Island, New York, on October 19, 1958. There were some 200 persons in the inaugural procession representing learned societies, educational associations and institutions of higher learning. At the request of the Executive Secretary, the undersigned had the distinct pleasure of representing the Entomological Society of America at the inaugural ceremony.

The greetings of the officers and members of the Entomological Society of America were conveyed to President Heindel.

MAYNARD J. RAMSAY
Staten Island, New York

THE FIRST INTERNATIONAL CONFERENCE FOR INSECT PATHOLOGY AND BIOLOGICAL CONTROL

From August 13 to 23, 1958, the First International Conference for Insect Pathology and Biological Control was held at the Hotel International in Prague, Czechoslovakia. Approximately 150 delegates from 20 different countries attended. There were two delegates from the United States: John D. Briggs of the Illinois Natural History Survey, and Edward A. Steinhaus of the University of California. The latter was assigned the role of spokesman for the Western nations while I. A. Rubtzov of the USSR lead the delegation of the Eastern countries. The Conference was held under the auspices of Czechoslovak and Slovak Academies of Sciences, with J. Weiser, head of the Academy's Laboratory of Insect Pathology in Prague, and A. Huba, in charge of the Laboratory of Plant Protection in Ivanka, serving as host-chairman of the meeting.

The Conference opened with speeches of welcome by Professor Ivan Málek, Chief of the Academy's Institute of Biology, and by Drs. Weiser and Huba. This was followed, on August 13, by the beginning of the scientific sessions and the presentation of scientific papers, lasting through August 16. The following ten sections (and their moderators) were convened: (1) Insect Bacteriology (E. A. Steinhaus), (2) Insect Mycology (N. A. Telenga), (3) Insect Helminthology (J. Weiser), (4) Taxonomy of Entomophágous Insects (Z. Bouček), (5) Evaluation of the Results of Introductions of Entomophagous Insects (I. A. Rubtzov), (6) Insect Virology (G. Bergold), (7) Insect Protozoology (J. Weiser), (8) Rise and Effect of Parasitic Insects (P. Mesnil), (9) Use of Monophagous and Polyphagous Insects in Biological Control (Liu Chung Lo), (10) International Cooperation (J. Weiser and J. Huba).

A total of 65 papers were presented. Of this number 36 were concerned with insect pathology and microbial control, 23 with entomophagous insects and biological control generally, and 6 dealt with the matter of international cooperation in the fields of insect pathology and biological control.

In addition to the strictly scientific parts of the Conference, a sumptuous banquet was held the night of August 14, complete with appropriate toasts and gustatorial pleasures. Following the paper-reading sessions, several very enjoyable and greatly appreciated excursions were made. These included a sight-seeing trip through historical and modern Prague, a trip to Carlsbad and Marienbad to visit some of the famous Czech spas, and an extended excursion to Slovakia visiting Brno, Bratislava, the Tatras, climaxed with a frewell dinner in the High-Tatras.

It is impossible to describe and to evaluate the Conference in the space available here. Those interested in the subject matter of the Conference are urged to write to the Czechoslovak Academy of Sciences in Prague, or to await the publication of the complete Proceedings of the Conference in early 1959. Notable advances were reported by representatives of almost every country present. On the last day of scientific sessions conferences were held primarily between the delegates of Western and Eastern European countries to establish better cooperation and liaison between these two areas and the individual countries concerned. Representatives (J. Franz and P. Bovey) of the Western European Commission Internationale de Lutte Biologique (C.I.L.B.) explained their program and objectives. Delegates from Eastern European and Asian nations formulated resolutions and statements of intent to form a somewhat similar organization among their countries. The hope was expressed that eventually a closer liaison and cooperation could be established between the Eastern organization, the C.I.L.B., the Commonwealth Institute of Biological Control, and perhaps other groups.

Although regional meetings and conferences dealing with insect pathology and biological control have been held in several parts of the world, this is the first gathering of so large a group of scientists in these disciplines from so many countries. For this reason, this "First International Conference" is of historic as well as of current scientific importance. The exceptionally fine and expertly managed facilities, including a radio-earphone interpretative system providing simultaneous translations into Czech. German, English, and Russian, and the generous hospitality and solicitude of the Czech hosts, made the Conference one that could be enjoyed from a human-relations as well as a scientific standpoint. Opportunity was provided for numerous small conferences and meetings permitting valuable discussions and interchange of ideas and viewpoints. It was a Conference that no one present is likely ever to forget, and it is to be hoped that the place and time of the Second such Conference may be decided soon.

JOINT FAO/OIE PANEL

The formation of a new Panel on Tick-borne Diséases was announced in July 1958 by the Food and Agriculture Organization of the United Nations. Two members of the Entomological Society of America, Harry Hoogstraal and C. B. Philip were selected as members of this Panel. The first meeting of the Panel was on November 24–29, 1958, in London.

COLLOIDAL PRODUCTS CORPORATION

Mr. E. H. Littooy, President of Colloidal Products Corporation, has announced the appointment of Paul M. Ruedrich as Research Director of the Corporation. Mr. Ruedrich is a research chemist of wide experience. Mr. Littooy, a member of the Governing Board of the Entomological Society of America, was chairman of the committee responsible for the excellent publicity and entertainment at the Salt Lake City meeting. Colloidal Products Corporation has been engaged in the agricultural chemical adjuvant field for over 35 years. This firm is one of our SUSTAINING ASSOCIATES.

ALABAMA PEST CONTROL CONFERENCE

The Alabama Association for Control of Economic Pests and the Agricultural Experiment Station of the Alabama Polytechnic Institute are sponsoring a pest control conference on the campus at Auburn Alabama on February 24 and 25, 1959. Additional information may be obtained from W. G. Eden, Secretary-Treasurer, Alabama Association for Control of Economic Pests, Department of Zoology-Entomology, A. P. I., Auburn, Alabama.

POSITIONS OPEN—POSITIONS WANTED

Associate Professor, entomology and parasitology. Desires academic change. U. S. citizen, 40, Ph.D., C.A., P.&E., 15 years experience, numerous publications. Box 9.

Economic Entomologist. Ph.D. 10 years experience. Research or teaching, college or industry. Box 10.

Ecologist-Economic Entomologist. U. S. Citizen, 34, Ph.D. Desires research with college or university. Box 11.

BOOK REVIEWS

GUIDE TO THE FAMILIES OF MITES, by E. W. Baker, J. H. Camin, F. Cunliffe, T. A. Woolley, and C. E. Yunker. Contribution No. 3, Institute of Acarology, University of Maryland, College Park. 242 pp. June 1958. Price $5.00.

Mites have become the subject of intensive study during the past few years, primarily because of increased recognition of their importance in relation to agriculture and public health. Basic research on their phylogenetic relationships is receiving the attention of an ever increasing number of acarologists. The classification originally proposed by a few European scientists is undergoing considerable revision, and the new evaluations are being augmented daily by the careful study of old and new forms.

Thus the valuable conspectus of the Acarina, "*An Introduction to Acarology*" by Edward W. Baker and G. W. Wharton, published in 1952, is considerably out of date with regard to our present familial concepts. The "*Guide to the Families of Mites*" is a comprehensive treatment that brings us up to date.

Each of the major groups of mites has been treated by a noteworthy specialist: J. H. Camin (Mesostigmata), F. Cunliffe (Prostigmata), T. A. Wooley (Oribatei), and C. E. Yunker (Acaridiae). Edward W. Baker has been responsible for the overall treatment. The Hydracarina are considered to be a very specialized group, and they are omitted.

New Keys are presented to the families and higher categories. The superfamilies and still higher categoties are briefly characterized from a morphological standpoint. And an example of each of the 125 families considered is illustrated with the clarity, accuracy, and artistry of Edward W. Baker's pen. Comparative morphological features of the suborders are well delineated, but Camin's three-dimensional drawings of the mesostigmate genital plates are not reproduced.

Two additional features are included in the book. The first is a contribution by R. E. Crabill, Jr. on the comparative phylogeny of the subphylum Chelicerata. It is important that the specialist in a group of mites not lose perspective of the overall classification in which they belong, and Crabill has summarized it very well.

A second contribution is by Mr. Gordon Nunes who emphasizes the need for proper drawing habits to facilitate accurate reproduction of illustrations of mites. Such illustrations are now recognized as an invaluable tool of the mite taxonomist.

It is unfortunate that a few errors have crept into the "Guide". For instance, the drawings representing the females of the Alicorhagiidae and the Pachynathidae have been reversed, and the use of the number of genital suckers in the key to these and related endeostigmatic families is not a valid character. The illustrations of the venter of the male genito-anal area and the aedeagus of the Linotetranidae were inadvertently included under the Tuckerellidae where the male is unknown. But a few such errors do not detract significantly from the usefulness of the book.

A "Guide" to the families of mites was written and published in a form that particularly enhances the teaching of acarology, and it is an excellent contribution for those who teach this subject in various universities of the United States, Hawaii, and elsewhere. However, its usefulness is certainly extended to all of those who are interested in mite taxonomy.

A. EARL PRITCHARD

INVESTIGACIONES SOBRE ACRIDOIDEOS DEL URUGUAY. By Aquiles Silveira Guido, J. F. Carbonell Bruhn, Omar Núñez, and Enrique Valdés. Cátedra de Entomologia, Universidad de la República, Montevideo. 485 pp., 29 maps, 312 figs., 3 colored pls., 24.5 cm., paper bound. The book results from intensified biological and economic studies of the grasshoppers of Uruguay, conducted since those insects became extremely numerous and damaging in the early 1950's. It does not deal with species of *Schistocerca*, many of which have migratory habits which place them in the category of locusts, but only with primarily solitary grasshoppers. It is a comprehensive work, with much general information additional to that concerning the 28 species which are treated in considerable detail, and it should be most useful to Spanish speaking American entomologists. It is clearly one of the finest works on grasshoppers produced in South America. In fact, it places Uruguay on a par with Argentina with respect to grasshopper studies; these are the two South American countries where grasshopper research has reached a high level of accomplishment.

A general part (pp. 7–73) deals with economic importance and contains a well illustrated explanation of anatomical terms, a rather full summary of cytological studies of about 30 Uruguayan species, a brief review of ecological conditions at some 13 principal collecting stations, discussions of the proportions of the sexes and variation in the abundance of different species in various localities, consideration of natural enemies, and reports on tests of insecticides made in laboratory and field (including applications by air).

A special part (pp. 75–467) has an average of 14 pages for each of the 28 species discussed, and each species is illustrated by photographs and drawings. A systematic description of each species is followed by a consideration of its bionomics and importance. There are no identification keys except for the separation of subfamilies, nor is the book intended to be a systematic work, rather one with practical but at the same time fundamental uses. Pages 468–485 contain the bibliography and index.

It may be added that several recent papers of basic importance by Dr. Silveira Guido and various associates at Montevideo have dealt with the natural enemies of grasshoppers, with grasshopper cytology, and with the biologies of several species which have unusual oviposition habits or other pecularities. A set of the principal Uruguyan grasshoppers has been deposited in the U. S. National Museum through the kind cooperation of Uruguyan entomologists.

ASHLEY B. GURNEY

GRANDFATHER STORIES, by Samuel Hopkins Adams. 312 pp. Random House, New York, 1955.

During his boyhood in the 1870's and early 1880's the author and other youngsters of the Adams family paid regular visits to their Grandfather Adams, who lived near them in Rochester, New York. Grandfather Myron Adams, "born at the rag end of the eighteenth century," had operated a stump puller with 8-foot wheels during the construction of the Grand Erie Canal. When the Canal was finished he obtained employment on it. His pride in the canal and his affection for the people who made and operated it were without limits. The book "*Grandfather Stories*" is made up of the old gentlemen's reminiscences, chiefly about the Canal. Myron Adams' excellent memory was reinforced by occasional references to his ort-book. The present item reports his experiences as a pest control operator on the Erie Canal during the hot summer of 1828.

One day when he was nearly 30 he made friends with two men who were resting on the berm drinking callebogus. They had with them a chinch-killer, a 5-foot wooden box on wheels with a metal firebox slung under it. "On top of the whole thing was a conical peak, formed by a coiled vent that made it look something like a beehive." The word "Eliminator" was lettered on the front.

In those days taverns, warehouses, coaches, and canal boats teemed with chinches, especially the boats. The two rascals used an effective approach to ensnare prospective customers. When a canal boat with a woman aboard hove in sight one of the men offered to take her picture free of

charge. When she agreed he unslung his camera obscura and traced on paper the reduced outline of the client reflected on the ground glass. Then the other operator colored the likeness while the first sold the woman on the idea of an insect clean-up. Five to 10 dollars was the charge and a written guarantee was given that no chinch, cockroach, ant, spider, or weevil would survive. The Eliminator was carried to the hold and lighted, and all hands went ashore for a while. After a 3-hour exposure, doors and portholes were opened and all insects appeared to be dead. According to Grandfather Adams, the formulation seemed to be based on asafetida, with suggestions of sulfur, lime, pukeweed, skunk cabbage, and camphor.

For seventy-five dollars a third interest in the enterprise became the property of Myron Adams, who figured that the profit involved in this method of pest control was 90 percent. The three of them set out for Buffalo along the Canal, doing a fine business on the way. Upon arrival the profits, amounting to $600.00, disappeared along with the original partners. On his return journey east, Grandfather was berated by every canal-boat captain he met.

The bugs had revived after having been comatose for some hours. Grandfather drew $600.00 of his savings from the bank and repaid all the amounts that had been collected. And "it was a full three years before I convinced the Canal that nobody could call me Bed bug Adams without peril to his front teeth."

There is a sequel to Grandfather's 1828 experience with cameramen. In the 1880's his son, Myron, Junior, was talking in his office with a bank clerk who had invented a small box camera. Myron, Jr., was offered a share in the development of the invention. A contribution of $100 was suggested. Grandfather found out about the offer and roared that in his experience cameramen were rapscallions and that the inventive visitor was a higgler. Myron Adams' warning to his son about the little black box "with the Eskimo-sounding name" as the reason that Myron, Junior, turned down the offer of young George Eastman.

The author, now 85 years old, lives near Auburn, New York, on Owasco Lake.

PEREZ SIMMONS

INSECT HUNTERS AND TRAPPERS. By Ross E. Hutchins, 96 pp., illustrated. Rand, McNally & Co. 1958. $2.95.

STRANGE PLANTS AND THEIR WAYS. By Ross E. Hutchins, 96 pp., illustrated. Rand, McNally & Co. 1958. $2.95.

These two new and well illustrated books should strengthen the 4-H entomology project. The photographic reproductions are excellent and the books are well written. The former deals with insects such as ant lions, doodle bugs, assassin bugs, tiger bettles, fireflies, praying mantids and wasps. The latter illustrates how pitcher plants, venus-flytraps, lady's slippers and sundew plants capture insects, why jumping "beans" jump, how orchids, molds, lichens, misletoe and dodder live, the mystery of pollination and how seeds are spread.

M. P. JONES

INSECTS AND MITES OF WESTERN NORTH AMERICA. By E. O. Essig. xiii + 1050 pages. 774 figs. The Macmillan Company, New York. 1958. Price $18.00.

This is a revised edition of Dr. Essig's "Insects of Western North America," which was issued in 1926 and has been such an indispensable reference book (a real classic) for American entomologists so many years. A review of the first edition appeared in the Journal of Economic Entomology Vol. 19 (6): 878–79. 1926.

In the preface to the second edition the author refers to the phenomenal developments in insect control during 1944–46, and also to the great strides which have been made in systematic entomology, and states that although he has made many corrections and additions it would not have been possible to include all of the new and noteworthy data without writing an encyclopedia of several volumes. No one will disagree with this latter statement, but it seems regrettable that the present edition is enlarged by only 15 pages all of which were added in the indices. Ten of the 28 chapters were apparently printed without change and several others were changed only slightly. Pagination identical with that in the first edition was retained except as noted above.

In several instances the discussion of control measures given in the first edition has been revised and condensed and an additional illustration, usually of the insect species under discussion, has been substituted. In some instances, however, an illustration of some other species was included but these latter species were not included in the indices. It appears that more recent information could have been included without necessarily "writing an encyclopedia of several volumes", *e.g.*, the discussion of the alfalfa weevil ends with 1923.

This reviewer agrees with the reviewer of the first edition that it seems very unfortunate that Professor Essig did not follow more closely the list of common and scientific names approved by the Entomological Society of America. It seems hardly fair to the younger entomologist to continue the use of names long out of date and no longer accepted by workers in the respective families treated, *e.g.*, the potato leafhopper, *Empoasca fabae* (Harris), is still referred to on page 214 as the apple leafhopper, *Empoasca mali* (Le Baron) although the apple leafhopper complex was clarified by Ball in the JOURNAL OF ECONOMIC ENTOMOLOGY in 1924.

The illustrations are more effective than in the first edition, because of some additions and substitutions and possibly also because of the more suitable paper used for their reproduction. Pages 909–1050 complete the volume with very useful indices as follows: Index of Authors, pp. 911–21; Index of Host plants, pp. 922–36; Index of Subjects, pp. 937–1050. Few errors of any significance were noted in the printing. The book is very well bound for many years of use as a handy reference text and in that regard meets the high standards set by the MacMillan Company.

F. W. POOS

THE MOSQUITOES OF MINNESOTA, by A. Ralph Barr, 1958. University of Minnesota, Agricultural Experiment Station, Technical Bulletin 228. 154 pp. illus.

This is one of the best of the state manuals on mosquitoes. While primarily designed to facilitate the determination of the mosquitoes of Minnesota, now numbering 47, there is an excellent section on mosquito biology and another on techniques used in studying mosquitoes. The treatment of individual species also gives very fine and often extensive biological information. While keys are given for all species, the author's frank statements that the keys are not always to be relied upon are very refreshing and commendable. One of the best features of this publication are the excellent illustrations by Dr. Barr's wife. We feel however, that it would have been preferable to have had all the illustrations together to permit easier finding of any given figure. This is a very valuable contribution to the vast literature on mosquitoes.

A few minor errors might be pointed out. On page 36 there should be parentheses around Say's name; on page 99, couplet 3 of the key, the figure cited should be 79a, not 80. Finally, throughout the work, the name *diantaeus* has been changed to *dianteus*. There seems no justification for this.

ALAN STONE

MEMBERS IN THE NEWS

HERBERT MATSUMORI retired from the Entomology Research Division at Beltsville, Maryland and now lives in Hawaii. Aloha!

C. E. PALM, 1953 President of the Entomological Society of America, has been named Dean of the New York State College of Agriculture at Cornell University. Dr. Palm was Director of the Cornell Agricultural Experi-

ment Station and research director for the State Colleges of Agriculture and Home Economics. Previously he was Head of the Department of Entomology and Limnology at Cornell.

PHILIP B. DOWDEN of the U. S. Forest Service has been appointed technical advisor for the Federal forest insect biological control program in the northeastern United States. FRANKLIN B. LEWIS is acting leader of the biological control project and will be concerned with research on diseases of the gypsy moth.

Newly elected representatives on the Board of Directors of the National Pest Control Association include J. ARTHUR JONES, KENNETH W. TOMPKINS, EDWARD WITT, T. ROY YOUNG, JR., LEE C. TRUMAN, JAMES V. BENSCHOTER, ROBERT R. SCHENDEL, and WILLIAM I. ELO. The latter is a vice-president.

C. L. FARRAR has been named head of Bee Culture in the Entomology Research Division to replace J. I. HAMBLETON who retired in May 1958. Headquarters will be at Madison, Wisconsin. Mr. Hambleton recently returned from a trip to Europe.

W. R. THOMPSON, head, Commonwealth Institute of Biological Control in Ottawa, retired in June 1958. Dr. and Mrs. Thompson are visiting in Washington, D. C., and elsewhere while Dr. Thompson continues his studies of the Tachinidae of the West Indies.

H. B. HUNGERFORD, University of Kansas emeritus professor of entomology, and former chairman of the department, has been awarded the Joseph Leidy Memorial Award medal of the Academy of Natural Sciences of Philadelphia. Presentation was made on October 23, 1958 at the Academy.

HAMILTON LAUDANI and E. L. (MOE) MAYER have been transferred to the head office of Stored Products Insects at Beltsville, Maryland.

ED LITTOOY, a member of the Governing Board and a recent initiate into the Cardiac Club, is back on the job. Take it easy!

ELBERT R. JAYCOX recently joined the staff of the USDA, Bee Culture Section Laboratory at Logan, Utah. This laboratory is under the direction of G. E. BOHART who returned to Logan from Beltsville, Maryland earlier this year.

ALVAH PETERSON who recently retired from the Ohio State University, is now working with the State Plant Board of Florida at Gainesville. Pete would like specimens of insect eggs and invites correspondence.

E. A. STEINHAUS has been named Editor of the new *Journal of Insect Pathology*.

J. A. MUNRO who has been stationed in the Philippine Islands, is now in Rome with the International Cooperative Administration. Alex Munro and S. S. EASTER of the Velsicol Corporation in Japan, have been our roving ambassadors in the East. Steve Easter remains to carry the word in regard to the E.S.A.

GEORGE P. HOLLAND wound up a very successful term as president of the Entomological Society of Canada at their annual meeting in Guelph, Ontario, October 29–31, 1958. At the same meeting G. G. DUSTAN finished his equally successful term as president of the Entomological Society of Ontario.

JEROME G. ROZEN has joined the staff at the Ohio State University, Department of Zoology and Entomology. Dr. Rozen replaces DR. PETERSON who retired. He will teach courses on immature insects and on biological control.

ROBERT P. HOLDSWORTH formerly with the DuPont Company, has joined the Entomology Extension staff at the Ohio State University. Dr. Holdsworth will work in the fields of livestock, household, stored products, and cereal and forage crop insects.

JACK L. BISHOP has joined the Entomology Section of Virginia Polytechnic Institute. He will conduct research on insects of alfalfa.

C. W. FARSTAD, Officer in Charge, Crop Insect Section, Canadian Science Service in Lethbridge, Alberta, was selected to become Chief of the Plant Protection Division in Ottawa.

B. V. PETERSON recently accepted a position with the Canadian Department of Agriculture, Entomology Laboratory at Guelph, Ontario.

NEW PUBLICATIONS

LAND. THE YEARBOOK OF AGRICULTURE, 1958. U. S. Department of Agriculture, Washington, D. C. xi + 605 pp. plus two sections of pictures, one of 48 pages in the front and one of 16 pages between pages 384 and 385. For sale by the Superintendent of Documents, GPO, Washington 25, D. C. $2.25

LOCUST AND OTHER INSECT CONTROL, in Technical Cooperation Programs in the Near East, South Asia, and Africa, 1951–57. U. S. Department of Agriculture, Agricultural Research Service in cooperation with International Cooperation Administration. Misc. Publ. No. 770: 1–147. August, 1958. For sale by the Superintendent of Documents, GPO, Washington 25, D. C., Price 70 cents.

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